STUDIES OF QUALITY IN COTTON

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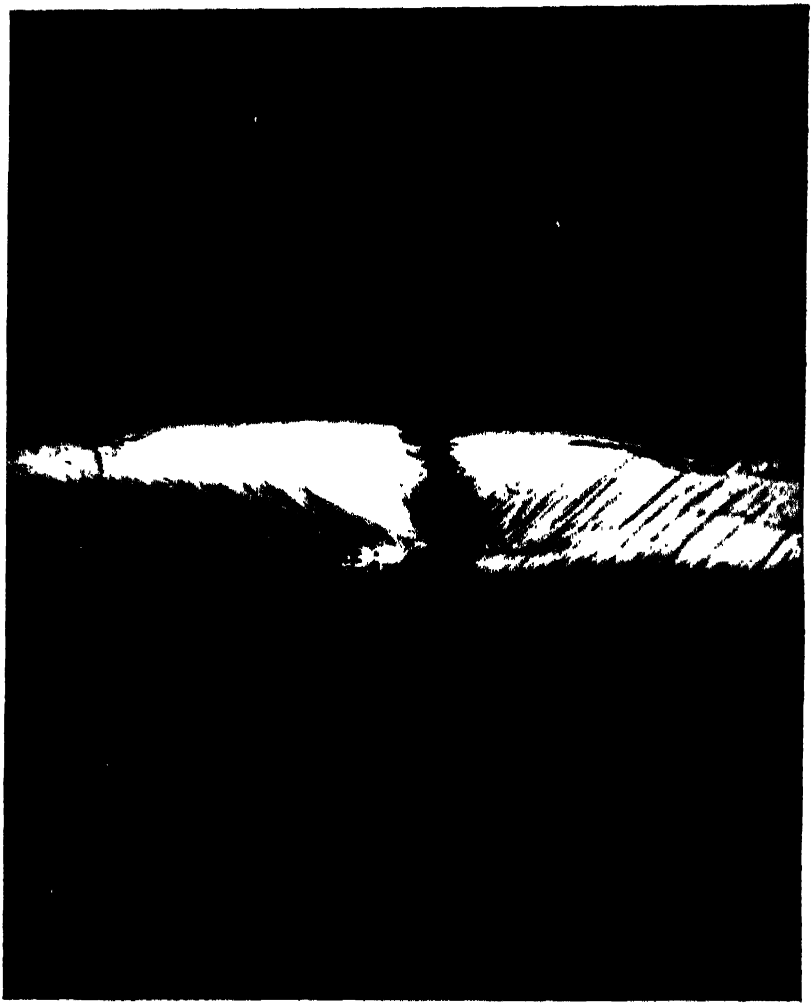

A Split Hair

Less than one per cent. of the total length of a single hair has here been split open lengthways, and one of the halves lies under the microscope, like a piece of an open rain-gutter. The cut edge of the wall is sharply focussed in the lower part of the photograph. The masses of fibrils which constitute the wall change over their spiral direction in the dark band which the use of polarised light has drawn across the hair. It is upon the tensile strength of these fibrils that the quality of cotton ultimately depends.

[*Frontispiece.*

STUDIES OF QUALITY IN COTTON

BY

W. LAWRENCE BALLS, Sc.D., F.R.S.

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BOTANIST TO THE KHEDIVIAL AGRICULTURAL SOCIETY OF EGYPT AND TO THE EGYPTIAN GOVERNMENT, 1904–1914, AND 1927
FORMERLY CHIEF OF THE EXPERIMENTAL DEPARTMENT IN THE FINE COTTON SPINNERS' AND DOUBLERS' ASSOCIATION, LTD., 1915–1925

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TO THE MEMORY

OF

JOHN WANKLYN McCONNEL

DEC. 20TH, 1913 MAY 23RD, 1922

PREFACE

The purpose of this book is to present an outline of the relationships which connect the properties of cotton yarn with those of the raw material from which the yarn is made. In that sense it is a sequel to an earlier book, *The Development and Properties of Raw Cotton*, cancelling and rewriting some ten pages contained therein. It is planned in a similar way, describing my attempts to analyse the essentials of Cotton-Spinning, just as its predecessor tried to deal with Cotton-Growing. Both books share in the inevitable defects of ambitious attempts. I can only hope that this new one will also be found useful.

Most of the scientific and industrial research on which the text is founded was effected under the auspices of the Fine Cotton Spinners' and Doublers' Association, Ltd., whom I can thank very sincerely for allowing me to keep alive the general studies which this book describes; a narrow-minded industrial combine might have demanded complete concentration upon those many problems of the Mill which can show an immediate effect upon the profit and loss account, though they may be trivial in the long run.

My great debt to the Association's late Chairman, and something of my regret for one who was a friend and a colleague, as well as my senior and Chief, I have tried to indicate by the dedication and in the text.

The interest taken by Mr. Vernon Bellhouse from the engineering side of the Executive, had a notable influence on the growth of our Experimental Department. Also, I am particularly indebted to the present Chairman, Mr. Herbert W. Lee, and to his colleagues on the Executive, for giving permission that this book might be published sooner than the time-limit originally agreed, in order that its views might the sooner be available for consideration by the Cotton-Grower.

Turning from my former chiefs to my former subordinates, I have to thank my ex-colleagues in the Experimental Department

for loyalty, friendship and enthusiasm; for trying to subordinate personal prejudices to the advancement of knowledge, as represented by the welfare of our Department; for helping me so to decentralise its affairs that I could eventually retire without prejudice to its work. Some account of their individual functions and contributions is given in the Introduction. Here I would only insist on the stimulus I obtained through daily intercourse with them.

To Mr. G. Udny Yule, F.R.S., I owe some mathematical help which eventually became of crucial importance in the interpretation of my subject. For such knowledge of cotton-spinning technique as an amateur may acquire I am largely indebted to the patience and expert knowledge of Mr. Walter Hamer. Some of my cotton-grower friends in various countries overseas may be able to make out traces of their contributions to my results, through the wealth of critical raw material which they provided.

Lastly, though this book appears now by permission of the Fine Spinners' Association, yet that Association and its members are not responsible in any other way for the contents of these pages, which must be regarded as entirely personal interpretations of observed facts, and do not in any way carry the force of their practical authority.

W. LAWRENCE BALLS.

Rose Cottage, Meldreth,
Cambridgeshire.
December 10th, 1926.

"Rock Bank," Bollington, Macclesfield

[*Frontispiece to Introduction.*

INTRODUCTION

THE SPINDLE AND THE HOE

The dangling spindle with its whorl of clay is one of the oldest of the tools which mankind has invented. The hoe made out of a deer's antler, or from a dressed flint-stone lashed to its wooden haft by rawhide thongs, is another of our most venerable possessions. But these two were handled separately, even in the dawn of tool-using; the spindle by the women, the hoe by the men; the spindle was more important to the nomads, the hoe belonged to the settlers on the soil. In our time, and for a century past, this separation has been emphasised, and most especially emphasised for the workers in cotton. The hoe-users work in the sub-tropics, the spindle-users work in the damp northern lands. The hoe is still in the open air; the spindles are packed by thousands in serried ranks within factories from which the city's grime is barely excluded. The men who control the workers of the hoe must inevitably look on life quite differently from those who direct the whirling spindles. Thus it happened that when scientists brought the cotton industry within the ambit of their curiosity they soon found that the Hoe could not do its best for the Spindle unless the Spindle sent word and description of its needs to the Hoe. The scientists had to examine them both, and this book is a summary of some studies thus made by one who has had the unique opportunity of spending two successive decades first with the hoe, then with the spindle.

My research work began with ten years in Egypt on the plant and the crop, then extended to the raw cotton and its use, and so (in Lancashire) to the machines and methods used by the cotton spinner, and even to the human factor. At half-time in 1915 it was clear that the farmer could never produce good quality cotton to order unless the spinner became able to explain exactly what was wanted. It was also clear that such exact explanations were not forthcoming because the spinner's

expert knowledge of cotton and yarn was traditional, in no sense an analysed understanding of the inter-relationships between cause and effect. At this point the enthusiastic pessimism of the late Mr. John Wanklyn McConnel enlisted me to attempt such analysis. We set out to translate cotton-spinning into such terms as would enable the cotton-grower, and especially the cotton-breeder, to understand and to provide what the spinner wanted. Thus we hoped that the Grower might become able not merely to grow cotton of some sort which the Spinner would take or leave, as in our present wasteful system, but that eventually the Grower might know what kind of yarn his product was capable of making, before ever it came near a spinning mill. Indeed, we wanted to see the cotton industry as competent in the use of its raw material as are some other big industries.

My late chief's effort to start a university department for this kind of study was not successful, but he obtained facilities for a little laboratory to be attached to the Fine Cotton Spinners' and Doublers' Association at the end of 1915. At the time of my resignation in 1925 the original laboratory had been built up into an Experimental Department (p. 338) employing twenty-six persons in its laboratories, workshops and spinning mill.

The present book picks out from the work of this decade such pieces of new and old knowledge as are relevant to the original intention of Mr. McConnel. In the interval this intention had carried us much further than we had ever expected, not to mention our pursuit of such side issues as arose when we tried to help the associated mills in miscellaneous difficulties. We had set out to find what was the relation between Strength in yarn and the properties of the cotton used in its making. This concentration on the problem of Strength turned out to be correct enough, for it is the central technical problem of cotton-spinning; but we found to our surprise that the main difficulty did not lie with the comparatively unknown raw cotton, but with the long-familiar yarn. Nobody knew what was meant by "strength" in yarn, and nearly ten years had elapsed before I could fit the last few pieces into my jigsaw puzzle.

The circumstances and manner of my presentation of the results in these pages require some attention. The text was first written to provide the Fine Cotton Spinners' Association with

a permanent record of my views on the broad interpretation of our work; it was then rewritten for the present publication. An alternative plan would have been to dissect its contents into a series of separate papers in technical journals, but such presentation of details is now being done, and will be done in the future, more fully and more ably by an increasing number of scientific workers. My concern is rather to sketch the outlines interconnecting these details, as I have seen them among the masses of such available.

I think myself entitled to request my reader to remember that I have deliberately risked criticism, even to disproof in details, in order to effect this broad synthesis.

Moreover, while merely trying to interpret the spinner to the grower I have stumbled into a field of research on the fundamentals of textile technology. The result is a certain unconventionality of outlook on textile problems, so that features of great practical importance relegate themselves to insignificance. Such alterations in the balance of parts may easily give a false impression to the practically minded amongst my readers.

Apart from such difficulties and defects in the actual presentation I am satisfied that the present sketch, though only an outline sketch, is fairly comprehensive in its picture of the relationships between cotton and yarn; this while fully aware that an immense amount of detail will be inserted in the outline by subsequent investigators. We know at last how to set about predicting yarn properties from an examination of the raw cotton. Most of the working hypotheses which are set out in these pages were formulated so long as ten years ago, and have since been subjected to repeated trial by the experiments and criticisms of my increasing staff of colleagues in the Experimental Department.

Methods

I have next to introduce the reader to the methods employed during the work, and I may perhaps be permitted to claim that in matters of apparatus and of general technique they are almost entirely original.

The original text of this book dealt with the apparatus devised for each investigation concurrently with the topic investigated, but in deference to the reader I have removed all the verbal sketches of apparatus to an Appendix (p. 309), where I hope the notes on their design will be suggestively useful.

The major aspect of the method question is presented by the Experimental Department itself and by its staff. It has seemed to me that some account of the Department was worth inclusion in the Appendix (p. 338), partly on its own account, partly because of the part which it has played in the collection of material for this book. It was not a big organisation in comparison with other research institutions; we prided ourselves on economy, so that its annual expenditure never reached the £10,000 per annum which I had taken as my estimated limit; still it had some features of peculiar interest. It was a natural growth-structure, employing a wide range of personnel of both sexes, doing work of most varied nature, from molecular physics to growing its own cotton and spinning its own yarns, while constructing its own apparatus and experimental machinery. I possess two bobbins of yarn which were spun on a machine of our own invention and construction, from cotton grown by ourselves on a pure line stock of our own raising. Withal it was found that this varied personnel of scientists, mechanics and mill operatives could fuse itself into a team, thus permitting such extensive decentralisation that it became practically automatic (p. 324 *et seq.*). Thus the outstanding problem of any research laboratory solved itself, for the work became independent of changes in personnel.

Certain members of the staff have been more directly connected with the work here described. In the original text I followed the usual scientific procedure by noting in the text the occurrence of specific contributions by each one, while pointing out that as the staff increased

> "some of them were active discoverers, or at least tested out my working hypotheses. It is as the result of their assistance that I am able to put a certain amount of flesh on the skeleton of this book. While much of the text and many of the diagrams could have been made from personal work, done with my own hands, yet the interchange of ideas within the team has made it difficult and unnecessary for me to do so."

In the present reconstruction I have followed their expressed wish by eliminating such direct references (except a footnote when an original discovery is involved), and am indebted to them for quite voluntarily suggesting this improvement in the form of

the book, although it is obtained by concealing our joint operations, because the reader can hardly be expected to take much interest in family relationships. I must therefore deal carefully at this stage with the relation of each person to the work recorded, this being now my only opportunity, taking them in order of their seniority as members of the Experimental Department.

Mr. H. A. Hancock was my personal assistant for nearly the whole ten years, occupying himself in progressive degrees of usefulness from his school-days onwards. He is particularly connected with the microscope research (Chap. II) and with the statistical observations on mill machinery. All the photographs used were taken by him, and some of the earlier testings are his work. During the last three years he took charge of the greenhouse and of the " general laboratory," where a botanical outlook was fundamental to a great variety of occupations.

Miss E. Lanigan began in 1917 as stenographer and computing clerk. The manipulation as well as the computation of our testing work was gradually passed over to her, and on becoming head of the Testing Laboratory in 1921 she accumulated and co-ordinated our statistical records. I have drawn perhaps more extensively in these pages on the facts provided by her efforts than on those of any other assistant, such being inevitable from the nature of these facts. To her, and to her senior assistant, Miss M. Ashton, I am also indebted directly for their help in copying and compiling the actual diagrams in which a small selection from these facts is here summarised.

Dr. Mary Cunningham's contributions to this book, with those of her assistant, Mr. R. C. Brimley, are rather restricted on account of the nature of their work in the Chemical Laboratory, which dealt mainly with " finishing problems," practically disregarded here. Nevertheless her work on the modification of yarn properties in " finishing " has given me varied cross-checks and side-lights on normal yarn, though without contributing directly to the main thesis.

To Mr. F. Hutchins I am especially indebted for his administrative assistance, which relieved me of much routine, making it possible for me still to direct and to collaborate in the actual research work itself, though using other people's hands instead of my own. Of more direct consequence to this book was his other position as head of the workshops, in which his engineering

knowledge, craftsmanship and inventiveness collaborated with my amateur interest in mechanism. The large Sorter (Chap. IX), and much of the later work on drafting (Chap. VIII), were conjoint researches wherein ideas came from both of us, and he did all the work of putting these ideas to the trial of construction, either actually or on the drawing-board.

My successor as chief of the Department is Mr. F. P. Slater, who joined us in 1921 to start a regular Physical Laboratory, which my early work had shown to be the scientific centre of cotton-spinning, while my elementary knowledge of physics had become inadequate to its further development. Returning after a year's interlude in 1925 he became head of the Laboratory Section. His work influenced and was influenced by our studies of cell-wall structure, his especial contribution (Chap. III) being to our knowledge of its electrical conductivity and its flexibility.

Mr. E. R. Goshawk assisted the last named for two years, and succeeded him in charge of the Physical Laboratory. We then collaborated closely in studies of yarn tension during actual spinning and in kindred analyses of machine behaviour. His fundamental contribution to these pages is the Porometer study of hair dimensions (Chap. III and p. 359), which he has since developed independently far beyond the preliminary data here given.

While the direct research contributions made by Mr. T. Ross have not yet had time since 1922 to become conspicuous, yet I am much indebted to him for so quickly developing the technique of our experimental spinning mill that I was able during the last year or so to put many ideas to the practical test. Experimental spinning is a messy job with the unadaptable standard machines, and as head of the Spinning Section he had gone far on the way to overcome the needless limitations imposed by them on scientific methodology.

Miss M. Savill should be taken last, though she became my private secretary in 1921, acting also as librarian, besides editing and " publishing " all reports and other circulated papers of the Department. To her literary interests I am indebted for peculiar help in revising and improving the original form of these pages, and in relieving me of all the tedious part of book-making. We managed to " publish " a limited edition of two confidential copies, 130,000 words long and heavily illustrated, just three

months after writing the first sentence. The present text has also had the great advantage of her voluntary assistance in revision and correction.

The remaining fifteen persons have no direct responsibility for the ideas here set forth, but they all have in some measure a representation which is indirect, through their exchange of opinions, whether as operatives or mechanics, or in their maintenance of order and system within the Department. To each in their respective degrees I would tender my thanks.

Presentation of Results

The general plan of the book is simple. It begins by a preliminary discussion of the subject out of which my whole inquiry took its origin, namely, the present commercial methods by which the value of raw cotton is assessed. We then join on to my earlier books with a sketch of the life-story and growth-structure of the cotton hair, in a purely descriptive chapter. The following chapter is so different in style and intention that the reader should be warned in advance against taking it too literally, for it is frankly a premature attempt to generalise amongst a miscellany of information which is essentially coherent, though superficially disjointed; it may be many years before this chapter on the ultimate structure can be written veraciously by some molecular physicist; meanwhile I have risked its conscious sketchiness in order to hasten the process. After some statements about the dimensions of the hair, the next chapter deals with a subject as definite as the other was vague, being the procedure which is practically usable for gathering such restricted information about raw cotton as is desired in a testing laboratory; it is rather a new phase of the cotton industry, and is bound to develop. These five chapters of Part I complete the discussion of our raw material.

In the next Part we pass to the properties of Yarn. The centre of this Part comes at its end; indeed, the chapter dealing with "The Thirteen Samples" contains the nucleus around which the whole subject has crystallised. Merely to present in Chap. XIII the results obtained from a study of these thirteen samples of ordinary American cotton has involved the writing of a book before attempting it. The chapters preceding it lead up through various aspects and difficulties presented by yarn, clearing the way to this crucial and dull chapter. To emphasise its importance

it is written with direct reference to its diagrams throughout, instead of leaving the diagrams for independent examination as in the other parts of the text. It is perhaps advisable to point out that although these thirteen low-quality American yarns were used as a kind of last touchstone, they constituted merely the most thorough test I could apply to any method or hypothesis; the general argument of the book is applicable to the finest or coarsest cottons or yarns, and the experimental work has been done on material provided from the whole range of cotton-spinning and of cotton-growing.

The Third Part discusses the possibilities and limitations likely to be met when applying such suggestions as have arisen in previous chapters to the conscious improvement or control of quality by the cotton-grower.

The Appendix is, as there explained, a fragment of what a more audacious writer might have made into an alternative Third Part, to deal similarly with cotton-spinning. Instead, it is confined to the methods of attack on those scientific problems which are presented by the industry. So doing, it has widened its scope to include more general aspects of research technique, contracting from this to the special methods used for the work described in these pages.

In the arrangement of the illustrations I have tried to avoid some of the disadvantages entailed by thus condensing the results of research work, when these results have not previously been published in full. The text runs continuously, with the minimum of reference to diagrams, and in it I impose my personal views upon the reader, largely avoiding the inconvenience of broken pages. The actual data upon which these views are founded, or, rather, a small selection therefrom, are presented separately in the diagrams; these are provided with legends sufficiently full to enable the reader to concentrate his attention on the facts and to form his own opinion on them, without prejudice from me.

Outline of Conclusions

Perhaps the most important single fact about cotton-spinning, for the purpose of analytical thought, is this—that singles yarn is unstable. To try to measure the properties of singles yarn is like trying to catch the proverbial lightning flash by the tail. So, while it is a truism that the yarn must break at its weakest link,

yet that weak link may have been darting from one place to another along the yarn during all the time in which we were pulling on it. This is largely due to "readjustment of twist," and is of industrial importance because it automatically compensates to a large extent for those structural imperfections which are so striking a feature of even the best yarns. It makes cotton-spinning possible under circumstances where it would otherwise be impossible. The instability is largely obliterated when two singles are twisted together into a double, or twofold, yarn.

This comparatively stable structure of the twofold yarn can be utilised to infer the distribution of weak links which originally existed in its undisturbed component singles. This distribution can be traced back to imperfections of the drafting process; these imperfections are partly due to imperfect manipulation, but mainly to inherent properties of Wyatt's drafting-roller reacting on inherent properties of the cotton hairs themselves. One component of yarn-strength is thus seen to be the distribution of weak links; it may be called the "draftability" of the cotton, or the "strength-gradient" of the yarn, and its chief determinant is the slipperiness of the hairs; the more slippery, the better.

But once the cotton has passed the final drafting, such slipperiness becomes a disadvantage. We then need hairs which will adhere together with the minimum of twist—for twisting is costly—so enabling us to realise the assets which exist in the tensile strength of the hairs themselves. This is the Second Paradox of cotton-spinning.

I enunciated the First Paradox some years ago:—"the weaker the hair, the stronger the yarn." [1] Always provided that other things are equal, the weaker hairs will be the lighter hairs, and they will make the better yarn because there will be more hairs in the cross-section of the yarn at any given count. The larger number of hairs implies a better distribution of weak links in the act of drafting, because this distribution is primarily a function of the number of hairs.

Given any particular distribution of weak links after drafting is completed, and allowing for the factor of instability caused by readjustment of twist, the strength of singles yarn then depends on the strength of the average hair, concurrently with the number of hairs in cross-section, of course. This latter number is the

[1] *Raw Cotton*, p. 144.

reciprocal of the hair-weight, and might be called "fineness, by weight." Hair-weight is thus a very important factor in the determination of yarn strength, coming into the equation twice over. The strength of the average hair can be expressed in terms of hair-weight, so giving us "hair-strength per equivalent weight," and this corresponds to the maximum possible strength obtainable in the yarn from any given cotton; it may be called the "Intrinsic Strength" of the possible yarn. Always provided that each hair takes its share of the load.

Actually it appears that in ordinary lengths of good yarn each hair does usually take its fair share, again on account of read-justments in the spiralled packing of the hairs. Also, however, the extent to which they will adhere without slipping on one another will depend upon the twist-angle, whether this be fairly simple as in singles, or more complex as in twofolds. The twist-angle reacts with the hair-slipperiness factor already mentioned. Alterations of twist permit deliberate control of yarn properties; there are differences from one cotton to another in this respect, due chiefly to the slipperiness factor.

Therefore, once the fundamental difficulty of instability in singles has been overcome, the relation between cotton and yarn becomes quite reasonable and not very intricate. Fineness by weight acts twice over, as does also slipperiness, while the strength of the cellulose wall itself is the chief remaining factor; the one character of cotton which we have been able in the past to measure easily, namely, length, is definitely less important than any of these three.

For the better discipline of the research worker there are some slight disturbances introduced into the relationship by the machines themselves, besides the major influence of the drafting rollers. The self-adjusting virtues of cotton and yarn serve largely to minimise these, but we shall deal with the chief of them, viz. the "mule-draw effect" and "nip-ratching"; both were observed for the first time in the course of these studies. Generally, we conclude—that the technique of commercial spinning is very near perfection in its use of the existing imperfect machinery; that radical improvements can be foreseen, though the way to them is not always obvious; lastly, that the cotton itself dominates the production of yarn to a greater extent than is commonly realised.

General Considerations

To conclude this Introduction it may be useful to note why and how I have so far departed from the type of existing text-books on cotton-spinning. The first thing to notice is that the state of my subject is not ripe for text-book writing. That subject is the behaviour of the cotton itself, and it is quite a new subject, whereas the existing text-books deal with the machines, relegating any discussion of their action on the cotton to such an inconspicuous place as our ignorance of cotton has formerly made unavoidable. That ignorance has been partly the result of formidable difficulties presented to the student by the cotton itself, with which I am only too familiar. It has also been affected by the high specialisation of the industry, forming water-tight compartments wherein there has been nothing obvious to be gained by any study except such as is directed to increasing the output; a mill which has been spinning the same count from the same cotton for the same market for half a century has no interest in the cotton, so long as conditions remain stable. If the conditions change, and we shall see in Part III that a change seems very probable, the direction of interest will change also, and my treatment of cotton-spinning may then have some use to the spinner as well as to the grower.

Also, in the same manner that we have in these pages no interest about the number of teeth in a gear-wheel, so long as we can make our gear changes and obtain from them the predicted result, so we are interested in ideas rather than facts. I am very far from forgetting that particular details, such as decimals of pence per pound, make all the difference between success and failure in commercial undertakings.

Throughout this book the cotton hair is taken as the unit in which all spinning problems must be formulated and solved, and although this seems rather obvious it is really rather novel. As farmers, chemists or spinners, we have looked upon cotton as lint, as cellulose to be purified, or as bales of raw material; only the botanists have thought about it as discrete hairs until quite recently. Now, the uniform appearance of a lump of cotton is deceitful, for not two hairs in a thousand are reasonably similar; even in such a generous interpretation of similarity as would allow the reader to accept a filbert nut instead of a Kentish cob, not two in fifty are alike. From this it follows that we shall have

to wait until methods have been devised for sorting out this complex population into groups of similar hairs before we can reach complete solutions of several spinning problems; incidentally, those solutions will be of practical utility, as well as theoretically interesting, when we have attained to them. Meanwhile we are the victims of this variety; the cotton behaves on the average as if it were uniform, and the peculiarities of individual hairs are not notable until the final yarn is reached; a rough analogy may be drawn from the way in which odd fibres of jute cause trouble in the mill.

Deep down below such coarse things as the microscope can show us in the hair, we come to the molecular and atomic properties. The living cell has constructed its coffin in accordance with those properties. Thus it is not in the least a far-fetched assertion to claim that researches into the molecular structure of the cellulose units are fundamentally important to cotton-spinning; these units build up the fibrils, which are packed around the growth-rings, which make the wall of the hairs, whereof the "cotton" is composed. Work of this kind was kept alive in our Experimental Department merely because I was convinced of its value as an intellectual stimulus, and yet it now seems likely that the very future of the industry may depend from such studies. Cotton is grown to be spun; if it spins bad yarn it is bad cotton; bad yarn is primarily weak yarn; weakness in yarn is *ultimately* due to inadequate molecular cohesion. It is not at all impossible that adequate cohesion may in course of time be obtained with materials other than cotton, and by other methods of building than those employed in the cotton-plant; the artificial silks give a hint of this.

This brings us back again to the biological side. If I am not seriously in error when claiming that we can now see how to set about predicting the properties of yarn from those of the cotton, it follows that the capabilities of the cotton-grower will soon be notably increased. The cotton-breeder is given three measurable characters to control, in addition to that of staple-length; all three of these would seem to be individually more important than the length-character, both on experimental evidence and from simple reasoning. The simplest kind of selection work on the existing commercial cotton crop, with the improvement of these characters as an objective, will be remarkably interesting.

Thus I have hope that time will show the full achievement of Mr. McConnel's purpose; the useful knowledge of the grower and the spinner linked together, the men of the Hoe talking a common language to the men of the Spindle.

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* Photograph.

* Photograph.

* Photograph.

PART I

THE COTTON

CHAPTER I

A PRELIMINARY EXAMINATION OF THE GRADER'S JUDGMENTS

A STAGE preliminary to these studies of cotton and yarn was reached during my researches on cotton-growing in Egypt, when these had gone far enough to require expert opinions upon the quality of cotton samples grown or bred in various ways. Such wide knowledge and remarkable skill as was then put at my disposal by Alexandria caused me necessarily at first to feel very humble, and to accept without question the weight of authority and seniority which lay behind the Alexandrian graders' judgments, as also behind those of other countries which I have met in after years.

Thus I have seen sample after sample correctly assigned to the locality in which it was grown, even to its own village. Among some samples of curious pedigree I have had one picked out as resembling the old Jumel cotton, which had been extinct for half a century, and yet was retained as a definite memory by the hands and eyes of one who had known it when a youth. Elsewhere I have published a series of judgments on "strength" which showed that whatever the grader might mean when speaking of "strength," still his estimations usually fell smoothly into a series as that strength altered.[1] In course of time I even acquired the elements of the craft for myself, until my own opinions upon series of samples ran quite fairly parallel to those of the expert.

Thus I have seen so much of the grader's skill, and that under circumstances which tested his skill far more severely than daily routine, as to have no doubt whatever that the decisions of the grader are real evaluations of certain properties possessed by cotton.[2] Such properties are perceptible by many persons, are appreciable by fewer, and the appreciation is capable of practical

[1] *Raw Cotton*, pp. 195 and 198.

[2] Cobb, N. A., "Stapling Cotton," Nat. Assoc. Cotton Manuf. (U.S.A.), 99th Meeting, 1915.

use only by those who combine perception and appreciation with years of daily experience. Tactile and muscular perceptions are employed, as well as sight, and the impressions yielded by each are integrated, consciously and subconsciously, into a recognition of the individuality of each sample, which then is stored in the memory. It is not surprising that such a complex mental process should easily be thrown off its balance by having to work in a strange light, or by unusual amounts of moisture in the cotton: most students of the crop have known a grader who praised and purchased a damp sample, though casting out a duplicate which had been dried. It cannot be expected that grading should be infallible. Even now, when we have material for measuring the degree of its fallibility, we may well be surprised that its errors are not greater; compare it with the error involved in judging the weight of an animal, which is comparatively child's play.

Admitting the reality of the grader's judgments, and remembering that they necessarily involve a "subjective error," we come to the problem of analysing these judgments. What does the grader mean by "strength"? What relation does his opinion bear to the performance of the cotton in the spinning mill? On the face of things there would seem to be no justification for asking the second question, because the whole industry buys, sells and mixes its cotton on the basis of opinions formed by such inspection and handling; a foundation which has thus far carried the weight of millions of pounds of capital must surely be above suspicion? Leaving this for the moment, let us see whether any analysis of the grader's meaning is possible.

In *Raw Cotton* I made some preliminary suggestions towards such analysis, for by that time I had met with serious objections to the unquestioning acceptance I had at first given to these expert opinions. Samples from identical plants growing side by side could not be recognised as constitutionally identical, but had received such varied judgments as "highly commended" and "useless." Insignificant samples had surpassed strikingly handsome ones, when both reached the spinning test through Mr. McConnel's courtesy; a short cotton had similarly beaten several long ones, and a sample so unsightly that it was only spun because of its genesis had also falsified all expectations. Evidently some analysis was necessary, and in that book I was able to show that grader's "strength" had no direct connection with the breaking

load of the hair (*ibid.*, p. 108), while a provisional conclusion was drawn that the uniformity of the cotton was largely implicated. There seemed reason to think that the grader's "fineness" was largely influenced by hair-strength, and I quoted the effects produced by using sealing-wax to give a good grip on the cotton, as indicating that "strength" judgments also implicated the slipperiness of the hair. Having gone so far I decided "to leave the matter at this point until one of the graders shall also attempt to write a book" (*ibid.*, p. 144). Such a book is still awaited, and in the meantime my inquiries have made it even more desirable, for we can now discriminate between the effects which are produced on the yarn by the principal properties of the hair; we therefore need a form of expression by the grader which will describe the slipperiness, fineness and strength of the cotton as properties quite separate from one another, just as each is separate from length.

There is no likelihood that any elaborate system of hair-testing can ever supersede the grader's rapid handling in day to day commerce, however essential such tests may be in special circumstances, such as research, investigation in the mills, arbitration or plant-breeding. It is not improbable that the quick and precise method already available for the measurement of fineness by weight (p. 66) might become a routine addition to the grader's technique, for without much trouble or delay it eliminates the fineness factor from his judgments of slipperiness, besides helping to dissect his complex "strength." Slipperiness also might obtain a rapid test in the future, but for hair-strength or intrinsic strength the grader's fingers and wrists provide the most practicable apparatus for everyday use. Some kind of hybrid between the laboratory and the "north light" seems quite feasible, but no laboratory by itself could get the work done in the time available.

Having admitted the practical limitations of my own technique for judging cotton values, I may proceed to discuss those of the other side, forced upon me stage by stage during many direct experimental tests of expert opinion, made from 1913 onwards. Allusion has already been made to some examples of discrepancy; these led me to the broad conclusion that the grader was much handicapped, unwittingly, if asked to deal with a new cotton; such "newness" might be the result of breeding a new variety,

or equally the result of putting an old and familiar variety into a new environment, as Webber 49 was put into Iraq.[1] It would seem quite reasonable that the grader should require a year or two in which to build up his store of subconscious knowledge connecting appearance with performance. Moreover, even when he is working inside the actual spinning mill it is not too easy for him to trace this connection with exactitude, on account of the habitual mixing of cotton therein; an over-estimate which concerns only one-fifth of the components in a mixing is not easily detected, even if it be not counterbalanced by an under-estimate of some other component. Spinning of individual cottons—that is to say, of material grown from one variety on one site and picked at one time—is rarely practicable outside experimental trials, and even then the sources of error in ordinary mill experiments (p. 75) are manifold.

When direct and dependable experiments are available to check the significance of hand-and-eye judgment, it is not in the least unusual to find serious discrepancies. We can form some idea of the degree of such discrepancies in a simple case from some estimations of "grade" on fourteen American cottons, grown on the same estate, and ranked simply by their dirtiness. Low-grade American cotton is usually that which has been picked late in the season, and the same exposure to weather which has dirtied the lint and lowered its lustre has also tended to weaken the hair, so that low grade usually brings about low strength by indirect causation. If a low grade happens not to have been weathered, but is merely dirty through careless picking, then its yarn strength might be that of a high-grade sample, while its yarn percentage remains low.

The fourteen samples here cited were spun to determine yarn strength in the lea test, and the yarn percentage; the results are here given for convenience in paired figure, 31/77 implying a strength of 31 lbs. with 77% yarn. It is evident that in the conditions obtaining over one estate the two are closely connected, ranging from 31/77 to 16/62. As there are eight grades, one step in grade corresponds roughly to 2 lbs. in strength and 2% of waste. But the estimation of grade is clearly seen not to correspond entirely with the "quality" denoted by this close correlation.

[1] Thomas, R., and Sawhney, K., *Emp. C. G. Rev.*, I. 3, *et al.*

STRENGTH, WASTE AND "GRADE."

Highest Grade (two samples)	31/77	29/75			
Third Grade (five samples)	28/74	27/74	27/72	26/71	25/70
Sixth Grade (two samples)	22/66	28/65			
Eighth Grade (five samples)	21/64	20/64	17/62	17/62	15/61

The first sample in the third grade was clearly second quality, the last nearly fifth quality. In the eighth grade the first two samples show sixth-quality figures. Now the close connection between all the pairs of figures shows that the differences from sample to sample were real, corresponding to differences in weathering, or, in other words, to "grade"; the same connection shows the spinning test to have been reliable. Hence we may conclude that in this, the simplest judgment required about cotton, only seven out of the fourteen were placed correctly, while two were two grades out of their proper place.

The chances of error increase when more complex judgments upon quality are required; so also does the magnitude of the error made in extreme cases. For example, a cotton graded as super-Sakel should give under certain conditions a yarn strength represented by the figure 2,500 and upwards, whereas a third-quality Sakel would only reach 2,100. Yet one set of experimental spinnings within my experience was largely wasted because a special "control" sample, graded as super-Sakel, could not be coaxed up even to 2,200; instead of being better than first quality it was bare second quality.

Another example involved more than a hundred bales, fortunately including some experimental lots which enabled cross-checks to be applied to the mystery. Peculiar temporary weather conditions had caused the hairs to ripen abnormally in the field, so that the yarn strength of some lots dropped to a mere half of its normal value. In others, less seriously affected, the strength was still below normal. For simplicity we will consider only four of the latter which had been grown from a standard commercial variety in field crops; these were valued by four graders at 123, 102, 123 and 123 respectively, the normal being represented by 100; they spun to strengths of 94, 76, 91 and 93. It will be observed that the inferiority of the second sample in relation to the others was correctly identified. What was missed was the general inferiority of all the samples, though this was most emphatic.

I have cited a case where grading for quality failed in routine

judgment on a routine cotton, and another case where it failed when confronted with unusual weather in the environment of a routine variety. The reciprocal instance for unusual varieties in a routine environment can be taken from opinions on record concerning four pure lines of cotton which I propagated in Egypt.

Acting under instructions to supply a new brown cotton for replacing the deteriorating stock of Afifi,[1] my No. 77 was run up to two tons of seed in two seasons, while two other browns, No. 95 and No. 111, were also propagated as reserves. Further, to demonstrate that cotton equal in quality to Georgias and Floridas (then doomed by the advancing boll-weevil) could be grown in Egypt, No. 310 was developed as a side issue.[2] These pure lines were simple selections in every case from known varieties; Brown Yannovitch, Kirkii, Assili and Sultani respectively, whereof all but Assili had become extinct. A committee reported on twenty-five samples from these four stocks, grown in three very different localities, as follows:

No. 77. "We warmly recommend you to continue the cultivation of this variety.

No. 95. "Is a fine cotton, much inferior to No. 77, with shorter and weaker lint. It is related to Assili; we think that a good stock of Assili grown in the same district would give as good results, or even better.

No. 111. "For the same reasons, No. 111 (and No. 95) do not deserve further cultivation.

No. 310. "A Sea Island quality which cannot be recommended for Egypt."

Comparing these opinions with the spinning results obtained from some of these samples, and from crops grown in later years by Mr. H. C. Jefferys, expressed in Part III (p. 296) as figures representing the strength of the yarn, it is evident that opinion and reality did not accord. The good opinion about No. 77 was hardly justified, while the short staple of No. 95 had blinded the graders to the fact that it could make a yarn nearly as strong as Sakel grown alongside of it. A descendant stock selected from contaminated No. 310 by Mr. H. C. Jefferys is still on the market, and is the typical super-Sakel of Egypt at present.

[1] W. L. B., *Ag. Jour. Egypt.*, 1912.
[2] W. L. B., *Emp. C. G.-Review*, III. 4, p. 344.

COMPARABLE YARN STRENGTHS OF FOUR PURE LINES.

Year	1917.		1918.	1919.
Locality . .	(a)	(b)	(a)	(a)
No. 77	1665	2055	1797	1800
No. 95	1828	2307	2012	2074
No. 111	1515	1966	1739	1667
No. 310	2053	2298	2263	2457
Assili	—	—	1594	1655
Sakel	—	—	2085	2426

Leaving the complex involved in " quality " and the prediction of yarn value by inspection of the cotton, it should be noted that the trade designations for length of staple have been found so unsatisfactory in practice that they have recently been discussed by the International Federation.[1] Now, although No. 95 had given me the first of several hints that length of staple is much less important than is commonly affirmed, still it is at present the primary classification character on the cotton markets, and ambiguities in its expression are undesirable. Such ambiguities are threefold: firstly, different graders have different standards of " length "; secondly, having the same standards their opinions may not coincide; thirdly, such lack of coincidence is even more probable when the regularity has also to be assessed. My invention of the Sorter mechanism (pp. 45 and 361) gave a method yielding reasonably exact information about length, and the subsequent introduction of the Baer sorter has verified its exactitude.[2] Those who have thus become familiar with the real length-composition of a cotton sample can only wonder that any arbitrary statement of " staple-length " was ever taken seriously.

Another group of American samples will serve to illustrate the first two of these ambiguities. They were graded four times for length, twice in the U.S.A. and twice in England, to sixteenths of an inch; the results can be tabulated in thirty-seconds by correcting up or down for " full " or " bare." Averaging the table crossways we find that U.S.A. (*b*) and U.K. (*a*) had a common standard, while the other two differed by 5/32, or 12% of the length evaluated. Adding 4 to the first row and subtracting 1 from the last row, to bring all four into line, this rough correction for

[1] Int. Fed. Master Cotton Spinners, Vienna Conference, 1925.
[2] *Vide* various Proc. Shirley Inst., Mancs.

difference in standards still leaves discrepant opinions about some samples, *e.g.* *h* (39, 39, 40, 43). Such discrepancies are, in fact, inevitable when opinions are involved, rather than measurement, or when rough measurements are applied to such heterogeneous material as cotton without very strict precautions to avoid errors in sampling.

LENGTHS OF "STAPLE" IN THIRTY-SECONDS OF INCH, BY FOUR GRADERS.

Sample :	*a*	*b*	*c*	*d*	*e*	*f*	*g*	*h*	*j*	*k*	*l*	*m*	*n*	Average
U.S.A. *a*	39	35	36	36	35	35	36	35	34	33	35	35	36	35·4
U.S.A. *b*	44	40	39	40	39	39	38	39	38	38	39	39	38	39·2
U.K. *a*	42	38	40	40	39	38	40	40	38	38	40	39	40	39·4
U.K. *b*	44	39	40	42	40	38	44	44	38	37	41	38	40	40·4

When we bring the variation of length within a sample into our discussion, checking the grader's estimations by means of the sorter, we find discrepancies which are really remarkable. An intensive study of twenty-eight samples was undertaken in the Experimental Department, from which only the salient results need be quoted. Eight of these were official standards, representing successive thirty-seconds as judged by the grader; the sorting showed their actual length distributions to be very confused, not forming any clear sequence at all. The remaining twenty samples had been matched to these standards, and also had been judged twice more by other graders. A slight correction for differences of standard was then applied, after which it was seen that all the graders were in agreement concerning six of the twenty-eight samples; three of these were judged unanimously to be of one particular length, and the remaining three to be of another length. The sorting of these two groups of three samples each showed differences of a sixteenth inside one group, and of nearly three-sixteenths in the other. Grading to steps of thirty-seconds would therefore seem to be quite impossible, even if a very modest degree of probability is desired.

Though the examination of these twenty-eight samples was the most thorough test I have yet applied to the common estimation of length, their details need not be laboured further. An equally striking example is available in a graph which I published in the Handbook [1] of the sorter, and it has the further advantage of

[1] *Method for Measuring Cotton Hairs*, Macmillan, London, 1921, p. 38, Fig. 5.

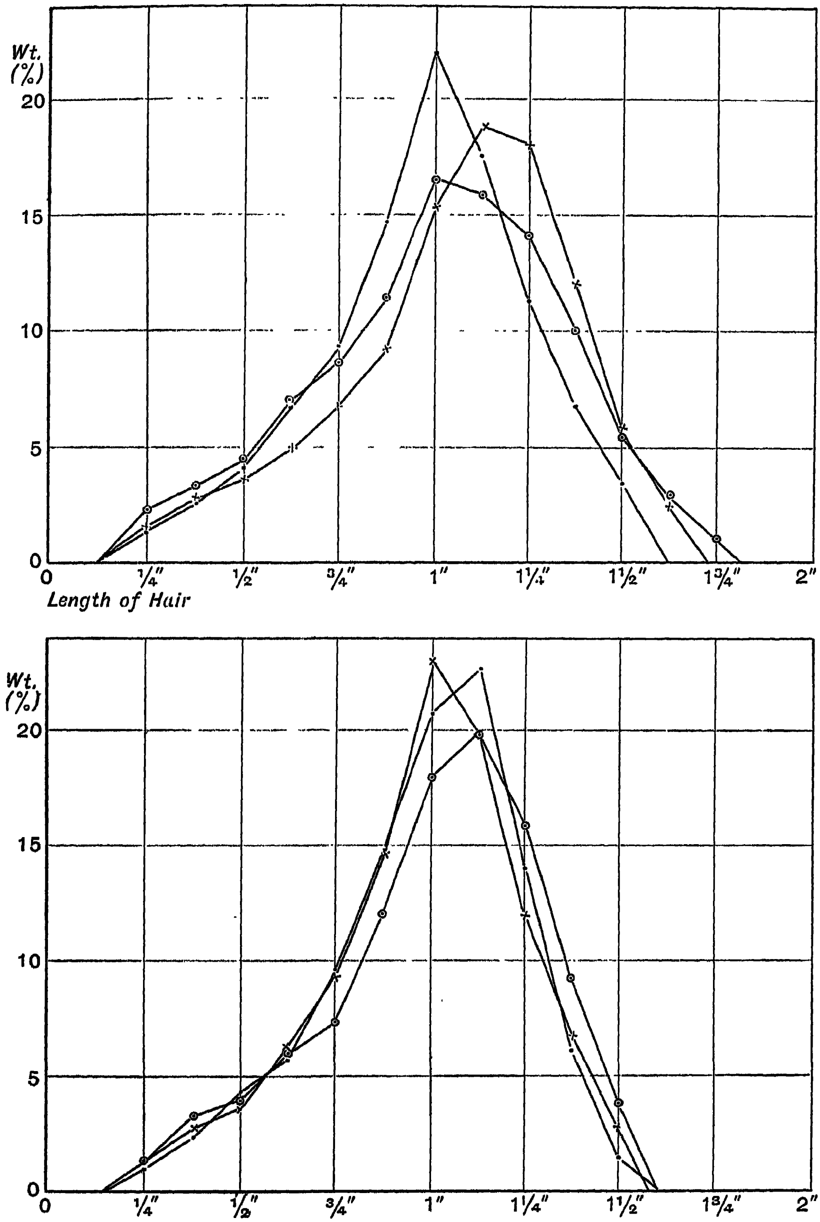

FIG. 1.—Comparison of hair length distributions in samples which had previously been estimated by graders to be identical in staple length.

being less elaborate. The graph in question showed the sorter composition of three standard samples, designated as $1\frac{1}{2}''$, $1\frac{5}{8}''$ and $1\frac{3}{4}''$ respectively. There is a clear and correct difference of $\frac{1}{4}''$ between the shortest and the longest, but the so called intermediate length is almost indistinguishable from the longest one. Differences of an eighth are counted as commercially important, and yet this mistake of an eighth was made under the following circumstances :—

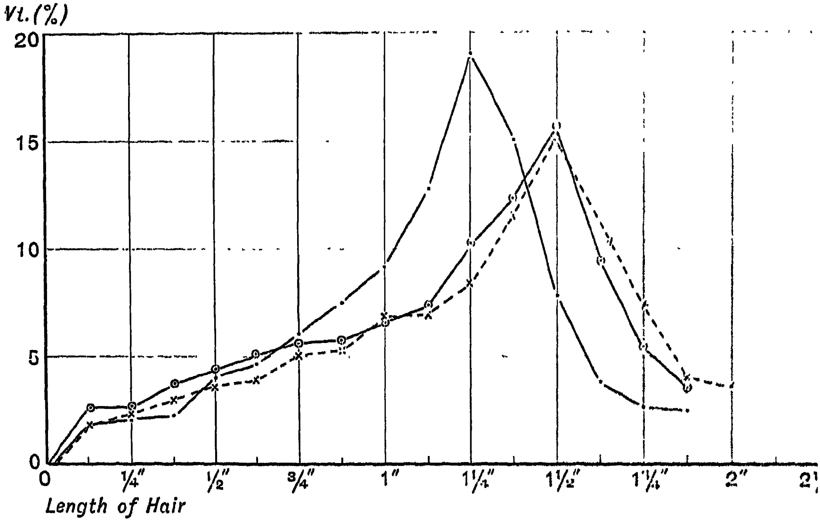

FIG. 2.—Actual hair length distribution accurately measured in three staple standards issued for Arizona Egyptian cotton, *circa* 1918. The nominal $1\frac{5}{8}''$ sample is barely distinguishable from the $1\frac{3}{4}''$.

There is no possible chance of misapprehension on either side. On the laboratory side the samples were halved, each half being separately sorted at different times, by different operators, on different models of the sorter; the whole of each sample was thus involved, and the duplicate examinations were in complete agreement. On the grader's side the samples were supplied in sealed packets, marked as being specifically staple-length standards for Arizona-Egyptian cotton, officially prepared and distributed about the year 1918 by the U.S.A. department of Agriculture.

When the graph was used in the Sorter Handbook for illustrative purposes, I did not specify the origin of the samples. I do so now, not in jest or malice, but in order finally to drive home my point, that hand and eye grading at its very best is unreliable.

Examples of this unreliability could be multiplied. So also could examples of brilliantly correct inferences made by the grader, like those which were mentioned (p. 3) as having impressed me so much in my first contacts with his craft. It is not, however, an easy matter so to write this chapter as to produce in the reader the desirable attitude of scepticism towards grading and to avoid giving an impression of superciliousness; therefore the examples already cited will suffice as a basis for attempting some rough estimate of the inherent subjective errors involved in the grader's judgments.

To do this we may with advantage glance at the elements of measurement in general. In most everyday affairs we reduce our measurements to money values, and are well aware that an error of a half-penny in thousands of pounds may keep a cashier at work all night. This absolute measurement in coinage units is apt to obscure the fact that relative measurements can rarely attain any high accuracy, whether in the laboratory or in the workshop. One of the most exact operations of the engineer is the manufacture of ball bearings, gauged as they are to 2/10,000″; even on a large ball, such as a quarter-inch one, we may call this an error of 1 in 2,500, which is equivalent to an uncertain penny on every ten pounds sterling. But exactitude of this order is rare in experimental work; any laboratory experiment whose results can be trusted to within 1%, equivalent to a penny in ten shillings, is reckoned quite good. Much experimental study has to be done under circumstances which prohibit the result of any single experiment from distinguishing clearly between the pre-war franc and the shilling; in these a frequent repetition of the experiment has to be combined with statistical treatment of all the results in order to arrive at the desired accuracy; measurements of crop-yield provide a typical instance.[1] We shall see later (Chap. V) that although the fineness of the cotton hair can easily be determined by its weight so well that differences equal to that between 9/6 and 10/- are sharply distinguishable, yet the hair-strength requires hours of repeated experiment before the existence of such a difference can be asserted.

Accuracy in measurement is thus a matter of degree, so that it is surely unreasonable to have expected exactitude from cotton-grading. Moreover, grading is not even measurement; it is

[1] Yule, G. U. and Engledow, F. L., *Emp. C.G. Rev.*, III. 2 and III. 3, 1926.

estimation, and anyone who attempts to halve a straight line, much less a crooked one, by estimation, can soon demonstrate statistically how much greater is the subjective error than the measurement error. Therefore, if we consider the matter without prejudice it is most eminently reasonable that such errors should occur as have here been cited for the estimation of grade, length, and for quality in general. It is to my mind rather a striking tribute to the general skill of cotton-grading that my worst examples should not be worse than they are.

But this still leaves us with a feeling akin to that engendered by an earthquake, when things previously regarded as immutable start to move about. Failing any measurements of known exactitude upon cotton, we have in the past had to trust the grader's hand and eye. Failing any direct check upon his judgment, that judgment has been our only "frame of reference" for cotton behaviour. That frame being found vague and flexible when compared with the less vague and wobbly frame of reference which direct experiment can provide, we nevertheless feel something akin to disloyalty in discarding it. Yet I hope I have made it clear that, apart from the convenience of obtaining approximate judgments very quickly, the grader's opinion can no longer remain the final court of appeal in cotton affairs. It is interesting, it is important if not standing alone, and it may be the only information available when time is precious and accuracy low, but it is so far from being infallible that ordinarily it is not quite certain about the difference between nineteen shillings and a guinea, while in circumstances of exceptional difficulty (p. 7) it may pay out a sovereign for a ten-shilling note.

Putting this in another way; though it is a method involving subjective error, though its decisions rarely obtain direct experimental verification in the mill for their betterment, yet the skill of the grader is so great that his decisions reach the same degree of accuracy as a good deal of experimental measurement. But it cannot improve its accuracy by statistical treatment, like the experiments. Its use is limited, and its place as the technical basis of the industry must be taken by the definite measurement of cotton properties. It is too flexible to serve as a stable foundation. Probably the future will develop a kind of hybrid technique in which the easier and quicker of the measurements available from the laboratory (Chap. V) will be used in the grader's office to

supplement a judgment admitted to be such, employed simply in default of something better.

Lastly, in regard to the special relation between the purpose of this book and the work of the plant breeder : there is no doubt now remaining in my own mind that the maker of new cottons could dispense with the grader's judgments through all stages of actual plant-breeding and selection. Those judgments are required at the finish only, and even then not to judge the real value of the cotton, which will already have been ascertained by spinning-tests following inferences from hair-tests, but simply to ascertain what the market thinks of it and will pay for it at the moment. This momentary value is quite distinct from the real value shown in actual spinning performance,[1] and the " moment " may last for years if the mere appearance of the cotton is such as does not please the buyers. The case of No. 95 may be cited again for an example where the selling price would possibly never have risen to the real value (p. 9), because it was comparatively short in staple; in any case, its yield was not good enough to warrant extended cultivation. Nor need our example be taken from mere research experience; it is known that crops of one particular cotton were bought for a long time on the open market at prices far below their real spinning value, which had only been detected by one consumer, and not, of course, disclosed. I have indeed come to doubt whether the grader's judgments have been of any real service to the cotton-breeder in the past; when errors have been made, such errors have led equally (*e.g.* p. 8) to the rejection of good cottons as also to the recommendation of bad ones. Now the former error was irrevocable, because potential successes have been thrown away for good and all; the latter is remediable, but only after the waste of effort involved in propagating eventual failures. Thus the grader's influence on plant-breeding has not even been neutral, but can be considered as definitely prejudicial to practical achievement. This may seem an ungenerous theoretical inference, but such has been my actual personal experience, already noted in this chapter. Thus, while the experts' judgment will of necessity continue to have its uses in other parts of the cotton industry, it can be no more than a thing of incidental interest (Chap. XV) to the maker of new cottons.

[1] W. L. B., *Handbook of Spinning Tests for Cotton-Growers*, Chap. V. Macmillan, London, 1920.

CHAPTER II

THE MICROSCOPIC STRUCTURE OF THE COTTON HAIR

The growing hair—Wall building—Growth rings—Convolutions—Spiral constructions—Reversals—Pit spirals—Slip spirals—Units of the structure.

THE importance of the single hair, as being the unit in which all spinning problems must be solved, was pointed out in the Introduction (p. xix). The need for definite measurements upon the cotton, in such terms of the single hair, was examined in Chap. I. We can now examine this hair, the unit of cotton-spinning, more carefully; first describing the structure which is developed in it by growth during its lifetime; then proceeding to measure, as best we can, such parts of the dead structure as are significant to the spinner.

Some knowledge of the botany and agriculture of the cotton plant can be assumed, for these have been treated by many authors. The present account can, therefore, begin at the stage where single hairs become obvious as individual living entities, thus overlapping this chapter with part of my *Raw Cotton*, and thence joining on to some subsequent publications. The whole story is summarised without more detail than the reader will require in order to follow the development of our topic.

The hair is formed by the outward extension of a single cell from the epidermis of the seed-coat; this extension, unaccompanied by any cell-division, may continue until the hair is two thousand times as long as it is wide. Such an attenuated shape is not easily realised; a single hair laid on a black background in a good light looks much more sturdy to the naked eye. In a lecture-room I have demonstrated it by drawing a line with an ordinary stick of blackboard chalk, which leaves a streak about three-eighths of an inch wide; if this line is carried from end to end of a thirty-foot blackboard, such as runs across the front wall of a lecture-theatre, it represents even then a mere thousand-

fold ratio of length to width; in other words, it depicts a short and coarse cotton hair.

The sprouting of such individual cells of the epidermis is not identical, although they all sprout at the same stage of boll-development, on or near the day when the flower opens. Some cells grow to full-sized Lint hairs, others to shorter lint; others elongate as little as thirty-fold, and so form the Fuzz hairs. The relative numbers which sprout in these different degrees upon various parts of the seed-coat are chiefly determined by genetic factors; different kinds of cotton plants thus show characteristic differences. If the lint be combed out to form a halo[1] round the seed, this halo has a characteristic shape; the simpler examination of seed from which the lint has been ginned also shows various degrees of length and pattern in the fuzz of different cottons. With regard to simple length only, the limits known may be exemplified by Dr. Harland's Sea Island, No. V. 135, which produces hairs longer than 75 mm., whereas the wild species, *Gossypium Kirkii*, has no distinction between lint and fuzz, the hairs barely exceeding one millimetre in length.[2]

The relative behaviour of the epidermal cells is determined mainly by heredity, but the absolute percentage of sprouting cells is perceptibly influenced by environment; hence arise seasonal variations in the ginning percentage.[3] Similarly, the absolute length attained by the sprouting cells is much affected by environment, though the relative length in different parts of the seed does not seem to be seriously affected.

The future career of any one cell of the epidermis is not merely a choice between growth and stagnation, but is between the alternatives provided by two kinds of growth. Those cells which do not become hairs by growing along one axis proceed instead to grow in several directions at right angles to that axis, so forming themselves into a supporting pavement of star-shaped cells between the bases of the hairs. We are ignorant as to the determination of these alternatives.

There is a sharp distinction between growth in length and growth in thickness. The boll cannot enlarge beyond a certain limit of size, fixed by unknown mechanism; as its growth slows

[1] W. L. B., *Yr. Bk. Khed. Ag. Soc.*, Cairo, 1905, p. 47.

[2] To Mr. Brand, of the Tanganyika Agricultural Service, I am indebted for live seed of this remarkable species.

[3] W. L. B., *Yr. Bk. Khed. Ag. Soc.*, Cairo, 1909, p. 96.

down, so also the enlargement of the seed and the lengthening of the seed-hairs slow down and stop. This happens approximately at half-time in the maturation of the boll. Growth has not ceased, but it behaves as if a switch had been thrown over, thus releasing a different set of growth machinery. The hair cells now proceed to consolidate the position.

Thus far the long tubular hair has had only a very thin wall composed of an inner layer of cellulose—the Primary wall—covered by an external film of cuticle, which is presumably covered by the wax layer. The living protoplasm which has built it, and keeps it distended by osmotic pressure, now proceeds to lay down on the inside of this wall a thickening deposit of a different cellulose, called the Secondary wall. This depositing process is commonly discontinuous, especially so when all growth of the plant is arrested during the afternoon, as happens in most good cotton-growing countries; consequently the secondary wall is built up of concentric shells, which can be made visible as growth-rings in transverse sections of the hair. These rings are analogous to the annual rings of a tree-trunk, with these two differences; that they represent each the growth-deposit of one night, and that the innermost ones are the last to be formed. The existence of these concentric shells, some thirty in number, must have some effect on the physical properties of the hair; but though I have used constant illumination of the plant in order to grow hairs with few growth-rings or none, the nature of this effect has not yet been isolated.

The extent to which the wall is thickened is of importance to cotton-spinning, since it largely determines the fineness of the cotton; it varies enormously from hair to hair (Chap. IV). Within a group of only a hundred hairs from the same seed we can find the wall-thickness varying four- or five-fold. Such variations might be due to arrest of growth after the first few growth-rings had been laid down, or it might be due to variation in the average thickness of all the rings in the wall; the latter seems to be the more usual cause. Secondary wall formation can fail altogether in the extreme case, so that the ripe hair consists only of primary wall. Hairs such as these, together with those which have but little secondary thickening, are industrially important because the primary cellulose reacts differently to dyes; further, these hairs are so flexible that they roll up easily into the knots and tangles

FIG. 3.—The daily growth rings in the wall of the hair, seen in cross-section of a hair, after the wall has been swollen sufficiently to bring the ring-thickness over the limit of microscopic vision.

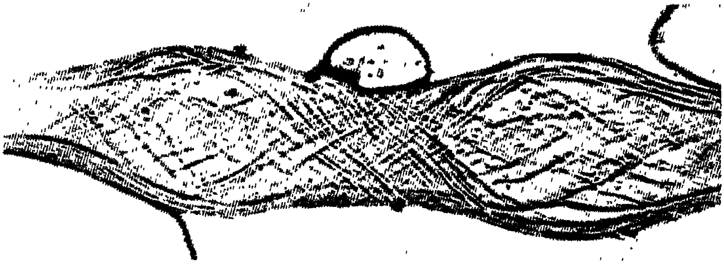

FIG. 4.—A hair which has been crushed to show the fibrillar structure of its wall.

FIG. 5.—Living, and therefore cylindrical, hair; viewed in polarised light. The direction of the spiral fibrils changes (left to right of photograph) from sinister to dexter, and back again to sinister. The two reversal places are shown up by the polarised light as dark bands across the hair.

[To face p. 18.

called "neps" in the mill (pp. 52 and 114). Such a "nep" is an artifact; indeed it is possible by appropriate ill-treatment to convert almost all hairs into such tangled knots; nep does not exist in the living boll, but is made by handling,[1] by ginning, and especially by the carding machine, from the hairs with unduly thin walls.

The secondary thickening is a restrained process in the lint hairs, rarely being carried far enough to obliterate the cell-cavity, and the boundaries between the growth-rings do not seem to be very sharply defined. As these rings are ultra-microscopic, and therefore invisible until they have been swollen by suitable chemical reagents, details of their finer structure are not easy to obtain. Still, their appearance is much less striking than that of the same rings in a fuzz hair, where they are comparatively thick and are sharply separated from one another by a boundary line of different material. A fuzz hair seldom forms more than eight to twelve growth-rings, by which time the cell-cavity is nearly choked; the fuzz thus differs from the lint in that it shows restricted length-growth with rank thickness-growth, whereas the lint grows freely in length but sedately in thickness.[2]

Eventually the growth-processes lead to the opening of the boll, and so to growth-cessation. The hairs thus exposed dry up and die. Removal of the internal support given until then by the turgid lining of protoplasm permits the cell-wall to collapse as it dries. When collapsing it falls into the familiar spiral convolutions, their direction being frequently reversed between right hand and left hand. The slope of these spirals depends in part on the thickness of the wall, while researches by Harland (*v.i.*) and ourselves have shown rather vaguely that both genetic and environmental causes can influence the average convolution shape in different parts of the hair. These familiar convolutions do not seem to be as important as is commonly asserted; we shall see later (p. 246) that no property of the yarn has yet been closely connected with them, though they may well play some minor part in determining the slipperiness of the hair; it would seem that any ordinary cotton is convoluted enough and that none are convoluted too much.

More interesting than the length of the convolutions is the

[1] Possibly even without handling, in the act of drying, on the plant in the field.

[2] The diameter of the fuzz hair is greater than that of lint hairs.

study of their direction and of the points where this direction reverses. The reversal-points are found to be coincident with, and determined by, the reversals of a spiral fibrillar structure in the secondary wall; the agreement between the two is perfect if the hair dries up without any constraint from adjacent hairs. This fibrillar structure was first discovered [1] by crushing the hair, but it is most readily examined by means of its appearance in polarised light. Any one growth-ring is built up as a tubular layer composed of about a hundred parallel but disconnected fibrils, and these are laid down in exact radial alignment with the fibrils of previous growth-rings; the secondary wall is thus perforated by some hundred or so of radiating sub-microscopic cracks or pits. It is possible that the thickness of one growth-ring can be built up of more than one fibril; in other words, the ultimate fibrils may be only a few molecules thick, instead of extending in this radial direction through the whole hundred molecules or so which make up the thickness of the growth-ring. We await further knowledge on this matter also.

The study by statistical methods of the reversing spiral which these fibrils form in the wall has been used to infer some further knowledge about the growth of the hair in length, a process which cannot yet be observed directly.

The study of " daily pickings " of cotton from a group of pure line plants was described in *Raw Cotton*. Further study of these samples revealed only one characteristic which was practically unaffected by the environment. This characteristic was the diameter of the tube in the living cell; it had to be measured indirectly (p. 66), and was only measured in the middle portion of the hair, but it directed my attention to the subject of cell-diameter and cell-shape. On examining the diameter in more detail along the whole length of the cell, making the measurements upon living turgid hairs, we find that samples from different kinds of cotton have different shapes from end to end; these shapes may be called " profiles." Changes in the profile may also be caused by environmental differences in one and the same cotton, but the chief interest of these profiles lies in the fact that all the hairs of one boll may have hairs shaped like an excessively attenuated flask of Chianti, another like a bottle of hock, and another may give hairs whose profile faintly suggests an elongated dumb-bell.

[1] By Mr. H. A. Hancock.

If we now take hairs of various lengths from any one boll, we find that after reaching the length of one millimetre (when growth at the tip has ceased) there is no change in the profile whatever the length may be. Thus if the biggest diameter is in the middle of a 5 mm. lint hair, it will still be in the middle for a 50 mm. lint hair, and so on. It is a reasonable inference that the shape can only remain thus constant during this enormous elongation if the new portions which are intercalated in the tube to lengthen it have been inserted quite at random, here and there, up and down the whole length of the cell; otherwise, growth must be entirely continuous all over. Testing the former assumption against the statistics obtained by measurement of the reversals, it seems clear that the reversals mark some of the places where such patches have been inserted.

The interpretation of the statistics was made easier by the curious fact that nearly every hair starts sprouting with a left-hand spiral structure; only six per cent. of them seem to begin with a right-hand spiral. When the hair first begins to differentiate itself from the neighbouring epidermal cells it must of necessity grow at the tip; having reached a length which is rarely greater than a millimetre or so, and may be much less, the growth-centre shifts away from the tip, and proceeds thenceforth to intercalate patches in the tube. In resuming operations after thus shifting from the tip it may perhaps insert another left-hand portion, and of this there will be no visible evidence if the tip-growth had been also left-handed, as is usual. If it inserts a right-hand patch, then the action shows up afterwards as a double reversal; left, right, and left again. So the building continues. (Fig. 20.)

We need an explanation why either hand of spiral should form at all, but again we are ignorant. All the observed results could be explained, however, if the growth-centre always worked round the cell in the same direction while it intercalated fresh material in places corresponding to each fibril in succession; as if it cut each fibril in turn, inserted a few score molecules of cellulose, and passed on to the next fibril. Then the hand of the spiral, right to left, would simply depend on whether the growth-centre, on beginning its second circuit, built in the next ring of insertions towards the basal side of the first one, or towards the tip of the hair. So we come to envisage the hairs in the growing boll as quietly squirming, like a miniature slow-motion Laocoön; any

one hair first twists one way until obstructed by its fellows, then shifts its growth centre and tries again.

The actual study of these processes is made doubly awkward by the present impossibility of observing them inside or outside the developing boll, and by the fact that these spiral structures are not visible to us until the secondary thickening begins, though they have been "predetermined"[1] during growth in length. They do not seem to have any practical significance as yet. We can find no measurable utility in the convolution-direction changes which they determine, and there is no particular difference between the longest cotton and the shortest. Their chief practical interest lies in demonstrating the remarkable individuality of cotton hairs. We have seen that there are differences from hair to hair which are quite big enough to be impressive, in wall-thickness and in length; we now know that even if we could pick out a thousand hairs which were closely similar in length and in fineness, it would still be most improbable that we should find any two of them alike in these details of the reversing spiral.

There is another spiral which the hair displays when suitably ill-treated; it has not been mentioned previously, because it is simply a consequence of the fibrillar structure of the pit spirals. Before doing so we may first summarise the wall-structure we have found, omitting now the reversal of the spiral. We can imagine ourselves exploring the wall-structure by boring into the hair with a sub-microscopic drill. If this drill were inserted radially it would either pass freely down one of the radial "pits" between two ranks of fibrils, after it had penetrated the primary wall; or, if it went into the secondary cellulose of a fibril in the outermost growth-ring, it would successively traverse the boundary and substance of some thirty growth-rings before emerging into the central cavity. Again, if the drill were inserted tangentially it would either pass between two growth-rings or through one, and in either case it would traverse successive radial cracks. Evidently the wall-structure is quite dissimilar in the radial and in the tangential directions, just like the wood of a tree-trunk. It remains to consider what our imaginary drill will meet with if we bore along the length of the hair.

We have found the fibrils to run spirally but continuously,

[1] W. L. B., "Analysis of Agric. Yield," Part III. p. 174. *Phil. Trans. Roy. Soc.*, B, 352.

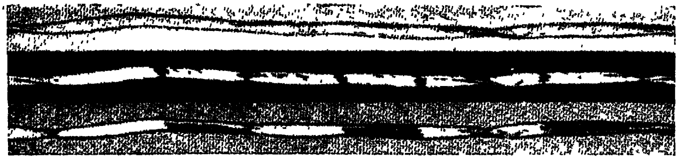

FIG. 6.—Three views of the same portion of a cotton hair. Ordinary light shows a half-convolution at the right-hand end of the upper photograph, but no other particular feature. The use of polarised light, as in Fig. 5, marks off five reversals by dark bands. The bottom photograph should be reproduced in the contrasting colours, such as emerald green and crimson, which are obtained by using elliptically polarised light; dexter portions appear as one colour, sinister as the other.

FIG. 7.—A hair taken out from an old piece of much-used cotton material, showing the "slip-spirals" which are thus developed as cracks in the hair, cutting across the fibrils. They also reverse their direction when the fibrils reverse, as here shown.

[To face p. 22.

from end to end of the hair, in spite of their reversals. Further, we saw them to be so strong that the wall may be crushed into separate fibrils without breaking them. If we now apply a different kind of stress by twisting the hair, or by bending it severely, a new artifact spiral is developed. Hairs taken from an old piece of string, or from card waste, show good examples of such stress-produced appearances; by suitable microscopic technique we can use them to measure the relative amounts of ill-treatment which cotton undergoes at each stage in the spinning mill. They were first noticed independently by Dr. Harland, through the changed appearance of the hairs in yarn which we had spun for him from his pure-line samples of Sea Island. These artifacts take the form of spiral cracks which lie at right angles to the pit spirals already described. Thus they also are radial in direction; but since the pit spirals lie at an angle to the hair axis which varies round about 27°, these artifact Slip spirals have a much slower pitch, making an angle of some 63°; this angle also reverses whenever the pit spiral angle reverses. They are termed "slip" spirals because actual side slip may take place under severe stress, like that of a fault in geological strata, and also because of the analogy between them and the slip-planes of crystals. They seem to be only a special case of structures found in other cell-walls.[1]

For the detailed examination of slip spirals the hairs should be mounted in some liquid which has the same refractive index as the hair; monochromatic light is used, and only one or the other of the two rays produced by double refraction in the hair; this can be isolated by inserting a Nicol prism in the eyepiece of the microscope (p. 349).

If we now mentally isolate a single fibril, the analogy with crystal structure is a close one. The optical axis follows the fibril axis through all its reversals, and the slip surfaces cut across the optical axis. It seems that slip surfaces develop at fairly regular intervals and are rarely closer together than twice the distance from fibril to fibril, though this is not at all certain. From the crystal analogy it seems clear that the bond which normally holds the molecules together on either side of a potential slip surface is molecular cohesion. But since the strength of yarn depends in the last resort upon the tensile strength of the

[1] For timber see Robinson, W., *Phil. Trans. Roy. Soc.*, B, 1920.

hairs, which in its turn is essentially the tensile strength of the fibrils, it is evident that we are here examining the molecular foundation upon which all the cotton industry has been erected.

Our present knowledge scarcely reaches beyond the bare recognition of this fact, but it will serve meanwhile to illustrate the importance of researches which might appear superficially to be quite disconnected from practical ends. In a later chapter we shall notice its possible implications in the development of artificial silk as a competitor against cotton; meanwhile we can complete our mental picture of the wall-structure by returning to the use of our sub-microscopic drill.

If such a drill were directed lengthways, not exactly up and down the hair, but tilted sufficiently to drill along a fibril, it would pass through successive layers at which the molecular structure was dissimilar from that in the rest of the fibril substance, the molecular bonds being weaker. Thus we may expect that the physical and chemical properties of the secondary cellulose would be different if we could examine them separately in the radial, tangential and longitudinal directions. Further, by these observations at the limits of microscopic vision we are given some reason to imagine that the cellulose is built up in units, each unit being approximately the shape of a flat brick or a domino card. The front and back of the domino are growth-ring boundaries, the sides abut on the pits, and the ends are slip-spiral surfaces. Our dominoes are all rather sticky on the back, very sticky at the ends, and clean at the sides. Each one is somewhere about 1/3000 mm. thick, or 0·3 μ. This comparison may help the reader to envisage the probable structure of a fibril.

I am not at all confident that these domino shapes are real unitary structures, though there would seem to be good reason to think that the living cell does build its wall with bricks of this kind. The unit may be much shorter than the domino, for slip spirals may be more frequent; the fibril between one pit and the next may be compound, as if the domino were made up from thin spills lying side by side; lastly, the individual growth-ring is in all probability a series of molecular layers, as if the domino had been sawn out of a piece of laminated ply-wood. However, it can be confidently asserted that such unitary structures exist, and that they cannot be larger than the limits indicated.

The next step in this research will be to get inside these limits;

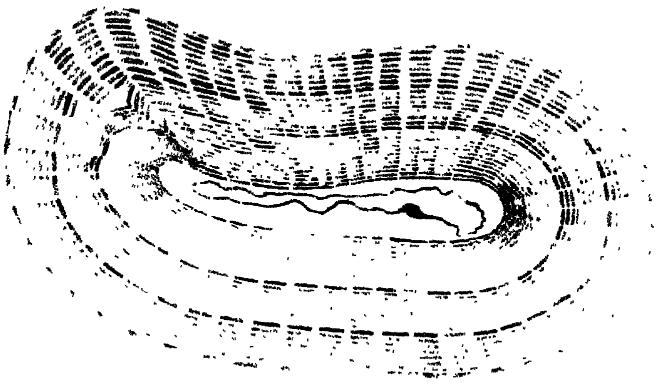

FIG. 8.—Diagram of the structure of an average cotton hair, as seen in cross-section. The dead remains of the living cell are shown in the central canal. This cell had deposited thirty growth-rings, each composed of a hundred fibrils, each fibril laid down on its predecessor in the previous growth-ring. Externally is the primary wall and cuticle. The crushing of the brick-like structure, which is shown at each end of the central cavity, took place when the original cylinder collapsed to its present form at the death of the cell; this crushed portion dyes more easily.

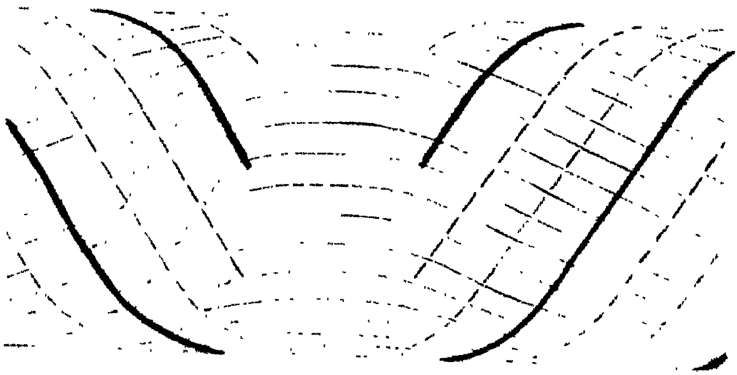

FIG. 9.—Companion diagram to Fig. 8, showing the pit spirals and slip spirals in side view at a reversal.

[*To face p.* 24.

trying to split up this structure, if it can be split further, into the real units or "micellæ." Then, investigating still further, we can attend to the arrangement of the molecules within the pseudo-crystal of each micelle, ascertain whether all the micellæ are alike, and in so doing come down to the atomic structure. This will probably be done fairly soon; possibly even before this book is published.

A beginning has meanwhile been made by linking together various studies by my colleagues on the relationships of dead hairs to water vapour, and connecting these with the coarser structural studies made under the microscope. Before discussing these in another chapter it is necessary to remind the reader that there is one big omission amongst the descriptions given in this one.

This hiatus relates to the surface structure of the hair, in the primary wall, the cuticle, and its wax. We know very little about them, though later we shall see that we need all the knowledge we can acquire, because of their relation to yarn properties (pp. 80, 248, etc.). The reason lies in the difficulties of observation and measurement; these require the development of special methods not yet invented. The thickness of the primary wall is on the limit of microscopic vision, and that of the cuticle is far less. We can, of course, recognise the familiar spiral constricting bandage which the cuticle forms when hairs are swollen, but we do not know with any certainty where the wax is situated, how it is arranged, whether the cuticle is pitted, or grooved, or plain, what are the details of its spiral pattern, nor what the structure and composition of the primary wall may be.

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CHAPTER III

THE RELATIONSHIP TO MOISTURE AS AN INDICATION OF THE ULTIMATE STRUCTURE

It is evident from the descriptions given in the preceding chapter that any knowledge concerning the molecular structure of the cellulose fibrils would be of fundamental importance to the cotton industry. It is only a question of time and resources before such knowledge is available, for direct methods are already available in the use of X-rays. It is true that such X-ray determinations as have yet been made upon cotton have given ambiguous results, but this would seem to be in part a natural consequence of the structure we have already described. The formal grouping of the unit components undoubtedly extends to dimensions which are greater than the inter-atomic distances dealt with in ordinary X-ray technique; it should therefore be advisable to extend and supplement such studies by the use of soft rays, of longer wave-length. Further, the spiralled arrangement of the fibrils necessarily implies that when a transmitted pencil of rays is employed, every right-handed placing of any unit will be counterpoised by a left-hand placing of a corresponding unit on the other side of the hair; this must produce a pattern on the photographic plate which is more complex than the isolated structure of either arrangement warrants. Seeking to avoid this statistical complication by using reflection methods instead of transmission, the same effect would again be produced, but this time by the alternation of right-hand and left-hand spirals at the abundant reversals. To isolate the effects due to the actual cellulose structure would not seem to be easy, since no random distribution is possible, excepting a distribution of the twinned fibrils which is analogous to a mixture of dextro- and lævo-rotatory substances in amounts exactly equal.

Nor is the hair a homogeneous mass of these spiralling fibrils of cellulose. The debris of protoplasm in the central cavity, the

mineral salts in this cavity and in the wall, the cuticle with its wax, and the amount of another kind of cellulose in the primary wall, all amount to an appreciable fraction of the total hair. No attempt to remove these by " purifying the cellulose " can be permitted, for such purifying treatment disturbs the original structure, which is the only structure interesting us, whether as cotton-spinners or as students of growth.

When I was considering the great desirability of embarking on X-ray analysis, it seemed to me that these difficulties would necessitate a great deal of special research, the production of unusual types of X-ray tubes, the planning of techniques for working with minute quantities of material, and generally the sacrifice of more time and attention than was warranted by the purpose and staff of the Experimental Department. Recognising the high importance of such studies I put aside the X-ray method with regret, but without dropping the subject entirely, for some inferences concerning the ultimate structure could evidently be obtained by studying the moisture relationships of the cotton. Moreover, any information about this relationship would be of direct usefulness.

The relation between cotton and the moisture contained in the surrounding air had already been studied fairly thoroughly, for this utilitarian reason, by earlier research workers; it had usually been measured by the means of changes in weight. The curves obtained in these gravimetric studies had an **S**-shaped or sigmoid form, while the one produced by drying the cotton showed a higher water-content than that obtained while the cotton was being wetted, thus enclosing between them a hysteresis loop. It seemed to me that the possibilities of the gravimetric method had not been exhausted, so some re-investigations were started, but these had not been carried very far before some new methods, much more sensitive, assumed greater importance. However, it will be useful to explain wherein the older work seemed to me to be incomplete.

The sigmoid curve is typical of many reactions. The growth of living things, such as the elongation of a cotton hair, usually gives a sigmoid curve. From studies I had formerly made, on the growth of a fungus in its relation to temperature, I had become familiar with the fact that this typical auto-catalytic curve could be divided into at least two portions, the steady acceleration

shown in the first part of the curve being presently opposed by a "depressant factor" which eventually dominates the situation and stops the curve from rising further. Comparing this experience with the weight-humidity curves obtained for cotton by Hartshorne or Schloesing, it seemed likely that there was some analogy between them. If the passage of the water into the hair be compared with the various paths of the sub-microscopic drill which we utilised in the preceding chapter, it becomes very probable that the oncoming of any new factor in the absorption will not be smooth and progressive. It seemed more likely that the weight-curve would show a "kink" in one or more places, if the weighing could be made sufficiently accurate.

Accuracy of the kind required was not likely to be obtainable by merely using very sensitive balances; it was quite likely that each single hair had in some measure its own individual manner of reacting to the atmospheric moisture. Ideally, the work should be done on single hairs—as was later effected in methods other than gravimetry—but the design of a dependable micro-balance for use under all conditions of atmospheric humidity involves difficulties which have not yet been solved. Meanwhile I improved the method by placing a moderately sensitive micro-balance of the torsional type inside a sealed chamber, loading it with only a few score hairs loosely fluffed out, and observing its deflections for weeks on end as the humidity conditions inside the chamber were successively changed. Though no data were obtained which were in any way good enough to demonstrate the existence of the suspected kinks, yet one important result emerged; though the few hairs used were freely in contact with the air, yet it took more than two days for the final equilibrium to be reached when any change had been made in the humidity; this although the initial disturbance of weight was very rapid. The obstacle to the uptake or loss of water was thus seen to be not in the slow diffusion of air through the masses of millions of hairs which previous workers had used, but was peculiar to the hair itself. Later, in other methods, we found that even single hairs exposed freely changed their properties just as deliberately.

The accidental difficulties which arose through trying to use such apparatus on an upper floor in a city street which was traversed by heavy traffic only permitted us to obtain one complete cycle of observations in many months, and the investigation was

held up, pending the transfer of our laboratories to the country Better methods than the gravimetric one then became available placing the existence of these kinks in the curve beyond doubt.

Before discussing the preliminary indications which these discontinuities of the curves have given us, when the changes in weight are considered in parallel with changes in cross-sectional area, in electric conductivity, and in flexibility, it is necessary that I should make some preface of cautionary explanation. I have already described this chapter in the Introduction as a "premature attempt at generalisation," and have stated my own consciousness of its sketchiness; it is certainly comprehensive, but in no way dependable; its subject is interesting, but the presentation is not sound. My reason for deliberately including it, nevertheless, is that the accident of circumstances caused these various observations from different angles to be made under the stimulus of a general interest provoked by our discoveries in the microscopic structure of the hair (Chap. II). There was consequently a definite unity underlying this diversity, such as might not happen to occur again for some years, and the minor issues were eliminated rapidly by personal intercourse as the work proceeded. Thus a sketch of the central topic becomes possible—though rash—long before the component researches are even approximately complete. This chapter has therefore been written only in the hope that it will prove suggestive, by showing the close inter-relation between several subjects which are ostensibly independent, yet all depend on our attainment to knowledge of the fundamental structure of cellulose.

The interests of the team of workers who co-operated in these studies ranged by imperceptible degrees from chemistry and physical chemistry, through physics and metrology, to microscopy and plant physiology. Among the prior researches which we utilised I would quote those of Trouton, Masson, Schloesing and Hartshorne, of McBain and of Irvine. Contemporary studies by Williams, Pierce, and Coward provided cross-checks, and the work of the Rothamsted Physical Department was stimulating.

The variables whose effect on the hair was studied in more or less detail were primarily the humidity; secondarily temperature, with due regard to the Masson effect, and the kind of cotton. The latter variable includes not only cotton of different species

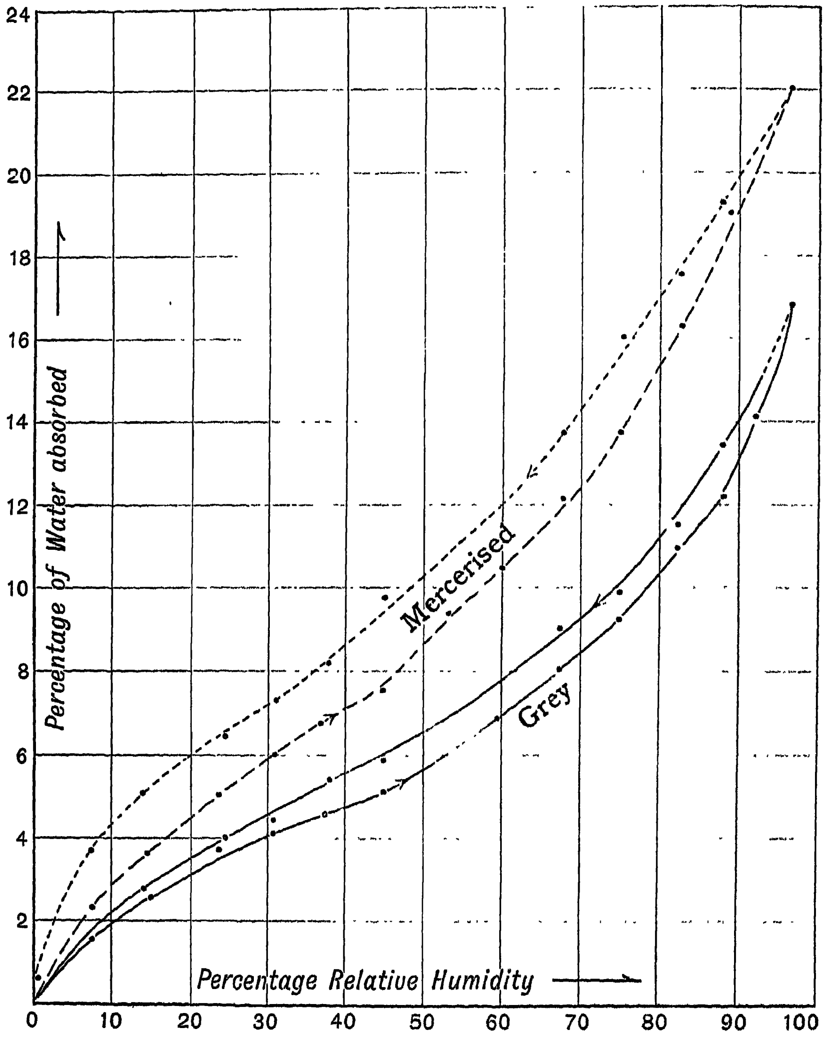

FIG. 10.—Approximate changes in the weight of a few hundred hairs, following changes in the relative humidity.

The hairs were suspended from a totally enclosed micro-balance, and two days were allowed to reach equilibrium after each change of humidity. The curve starts from desiccation over concentrated sulphuric acid, ascends by steps of about 7% changes to 94% relative humidity, and returns to 0%. The hysteresis loop thus obtained is much wider in the mercerised cotton, which also takes up 22% of water as against 17% by the raw cotton.

and different growth histories, but also the modifications produced by mercerisation, by carbonising, and by chemical change to cellulose esters.

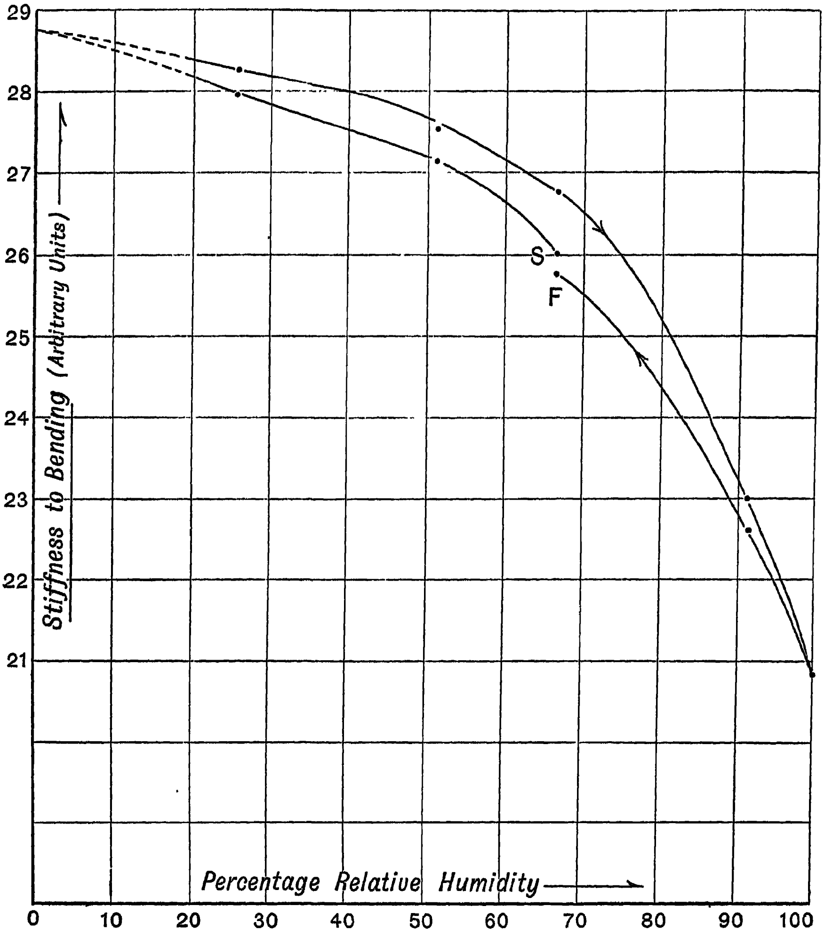

FIG. 11.—Approximate changes in the resistance to bending shown by a single hair, following changes in the relative humidity. The curve starts at 66½%, descends to zero, ascends to saturation, and returns to the starting-point. The hysteresis loop appears, and the dry hair is about 40% stiffer than the wet one.

The hair properties examined were the Weight, electrical Conductivity, Flexibility in bending and in torsion, the Dimensions in longitudinal, radial and tangential directions, together with the

cross-sectional Area of the hair by an independent method, and the heat phenomena shown in the Masson effect.

Notes on the experimental methods are given in the Appendix. It should be noted that only two of them reached easily to the standard of precision required for my purpose, these being the measurements of conductivity and of area. All of them aimed at sufficient precision to detect small local abnormalities in the curves obtained. These curves are of two kinds; first, the Cyclic type, in which stable values are obtained after prolonged exposure to a set of conditions, this being followed by change to slightly lower or higher humidity, and so progressively until the whole cycle from dry to wet and back again has been completed; the previous history of the hair is important in starting such curves, on account of the hysteresis effect; as each observation requires at least two days for the attainment of fair equilibrium, one complete curve of this type may take three or four months in the making. Secondly, we have the Time-curve type, which plots a record as nearly continuous as possible, to show the history of the measured characteristic after each alteration of the conditions, from the first moment of alteration onwards; these time-curves are specially useful because any deviation from smooth progression in their early course serves as an indication that some similar deviation may be sought in the more tedious cyclic curve. In the curves produced by either method the experimental data must be handled raw, unaltered by smoothing except as a last resort, since the general trend of the curve is relatively uninteresting in comparison with its details. The 40% region in the conductivity curves is an excellent example of this.

The Existence of Phases.—The cyclic curves for conductivity provided the first clear proof [1] that my suspicion of discontinuity was really justified. Whatever kind of raw cotton was used, the wetting curve always went through a double kink when the humidity was near 40%; for a little while the conductivity ceased to increase, and then resumed its increase at a faster rate than before. The drying curve on the return journey also showed a change of rate at the same point, but without the double flexure of the wetting curve. The delicate method employed was not in itself suitable for study of the whole enormous range of conductivity changes between 0% and 100%, so that the region

[1] By F. P. Slater

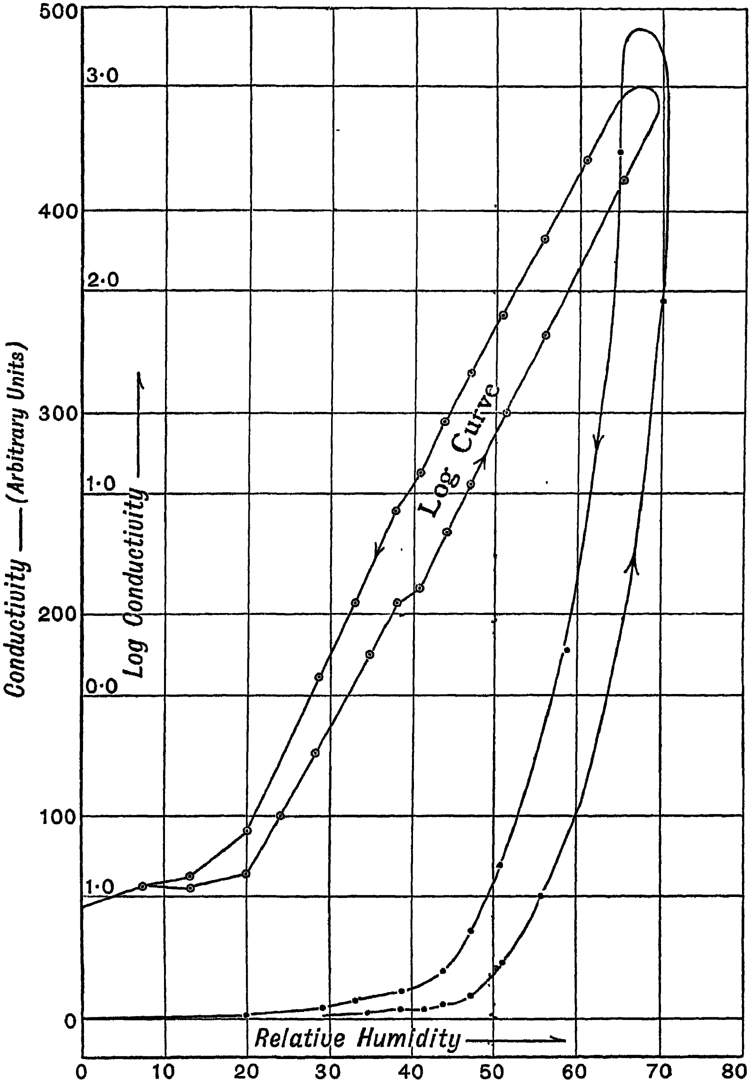

FIG. 12.—Changes in the electrical conductivity of a single hair, as the result of changing the humidity. One-day intervals between observations. The enormous change between 9% and 70%, shown by direct plotting in the lower graph, conceals the changes in form which are conspicuous when the curve is straightened by plotting the logarithm of the conductivity, as in the upper graph. The discontinuity at 38%–42% now shows differences in its form between drying and wetting (see Fig. 13), and the 15% region is clearer.

(From Slater, F. P., *Proc. Roy. Soc.*, B, 1923.)

between 20% and 70% was alone examined in full detail. This region being thus left "in the air," the possibility that other kinks existed in the curve outside this region was not investigated, though there was no doubt that the curve again changed its form below 20%. From the conductivity tests we thus ascertained that a kind of "phase-boundary" seemed to exist at 40% humidity, with the probability of another around 15%. The region above 70% remained unexplored.

The problem presented by exact measurements of the dimensional changes had for years seemed insuperable, in spite of much work on the subject. It was therefore the more pleasing to find that when once the right method had been made available for measuring changes in cross-sectional area (though without discriminating between the radial and tangential components of this area), the dimensional changes became even more noteworthy than those of the conductivity. The whole range from 0% to 100% could be explored in the same apparatus on the same material, with a degree of accuracy which was ample for our purpose. For the purpose of illustrating this chapter I shall use only the early results [1] with the Porometer which were available when the first draft of this book was written, but my former colleague's subsequent studies have greatly improved the demarcation of the phase-boundaries. A kink was located in the curve at 15%, thus confirming a suspicion engendered by the conductivity method. The 40% kink of the latter was corroborated emphatically, with the interesting addition that the same step-shaped kink was now shown in drying as well as in wetting. Another kink was located at 77% in the region which the conductivity method had failed to explore, while a faint indication which later work has fully confirmed was found near 90%.

We thus have good evidence that the mechanism by which water is taken up into the cotton is not the same at all humidities, but on the contrary is changed at certain stages of the increasing or decreasing content of water molecules in the surrounding air. The process passes through a series of Phases, whose boundaries are at values for the relative humidity which correspond roughly to 15%, 40%, 77% and 90%; it is possible that there may be other minor ones also. The location of the 40% boundary, at least, is definitely independent of the variety of cotton, whether

[1] By E. R. Goshawk.

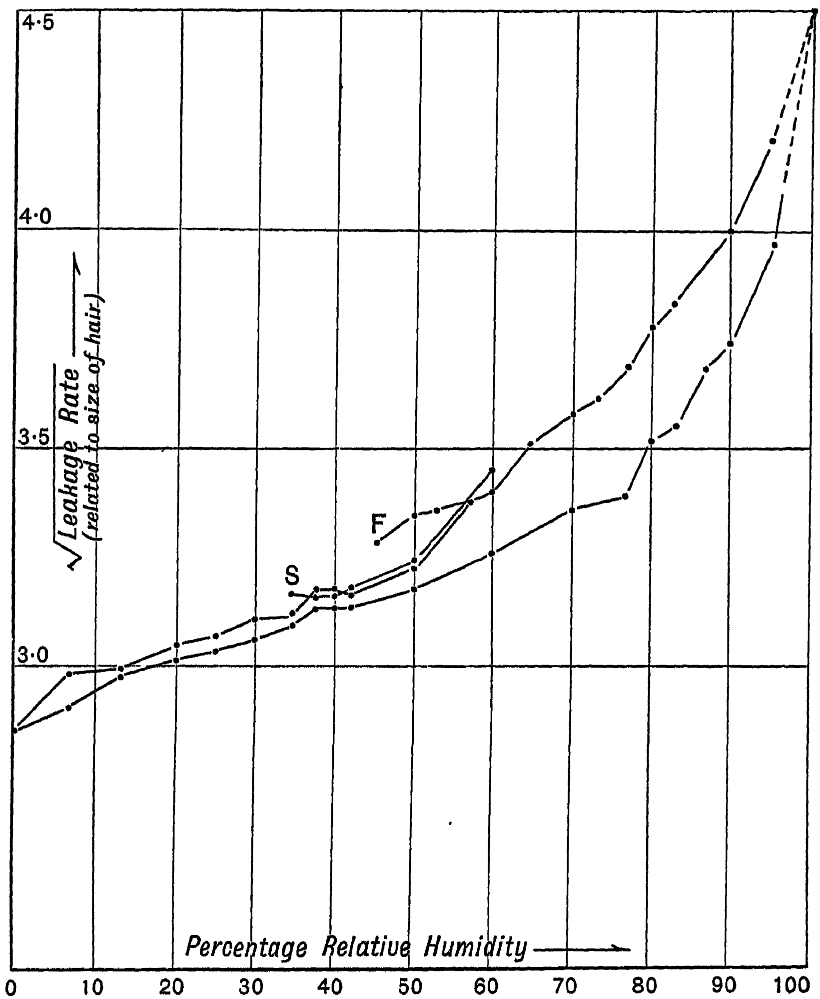

FIG. 13.—Preliminary measurements of the changes in cross-sectional area of a few hundred hairs, as inferred by the Porometer method, following changes in the relative humidity which were made at one-day intervals. This curve shows the first result obtained by a trial run round the cycle of changes. Discontinuities are suggested near 15%, in the 38%–42% region (see Fig. 12), at 70% and at 90%. The observations started at 35% went up to 60% to stabilise the plug system, and then returned to begin the complete cycle, mostly in 5% steps. Part of the varying width of the hysteresis loop depends on pre-treatment.

it be a rough Peruvian, Indian or Sea Island, as also it is independent of the dimensions of the hair.

Such phases are evidently to be interpreted as functions of the structure of cotton cellulose, whether in respect of microscopic, micellar, or molecular properties, or of all three. There is no one relationship of cotton to water, but at least five different relationships, possibly quite different in their respective causations. We may be dealing with a true chemical hydration in the 0–15% phase, with an adsorption in the 40—90% phase, and with coarse capillary phenomena above 90%, where we perhaps overlap with Coward's observations on the quantity of water retained by cotton after centrifuging.

Some analysis of the phases could evidently be effected by comparing the behaviour of raw cotton with cotton treated in various ways. For the purpose of this book it is sufficient to point out that such treatments have various effects, some of them not altering any of the phase-boundaries, others completely obliterating one or more of them. There is a big field for research in this direction.

Having regard to the variation of dimensions from hair to hair, and even within different parts of the same hair, it is somewhat surprising that the phase-boundaries should be so clearly defined. One would have expected them to be blurred as the statistical consequence of these variations; that the cyclic curves are not so blurred is an indication that we are dealing with more fundamental structure than that which is visible in the microscope. Even in the time-curves taken while passing across one of the boundaries, the passage is usually noticeable. Further, the micellæ have different properties on two of their three pairs of faces, if not on all three, as we saw in the previous chapter, and it is likely that an appreciable amount of redistribution of water may take place inside the hair after gross equilibrium has been attained.

Molecular Properties.—Some simple observations showed that the absorption of water by cotton was not merely mechanical. The vapour of carbon bisulphide, benzene and nitro-benzene is only taken up to the extent of about 1% under saturated conditions, whereas water and acetic acid rise to 18%, while intermediate values are attained by alcohol, acetone and ether. These were interpreted [1] as suggesting the possibility that molecular

[1] By R. C. Brimley.

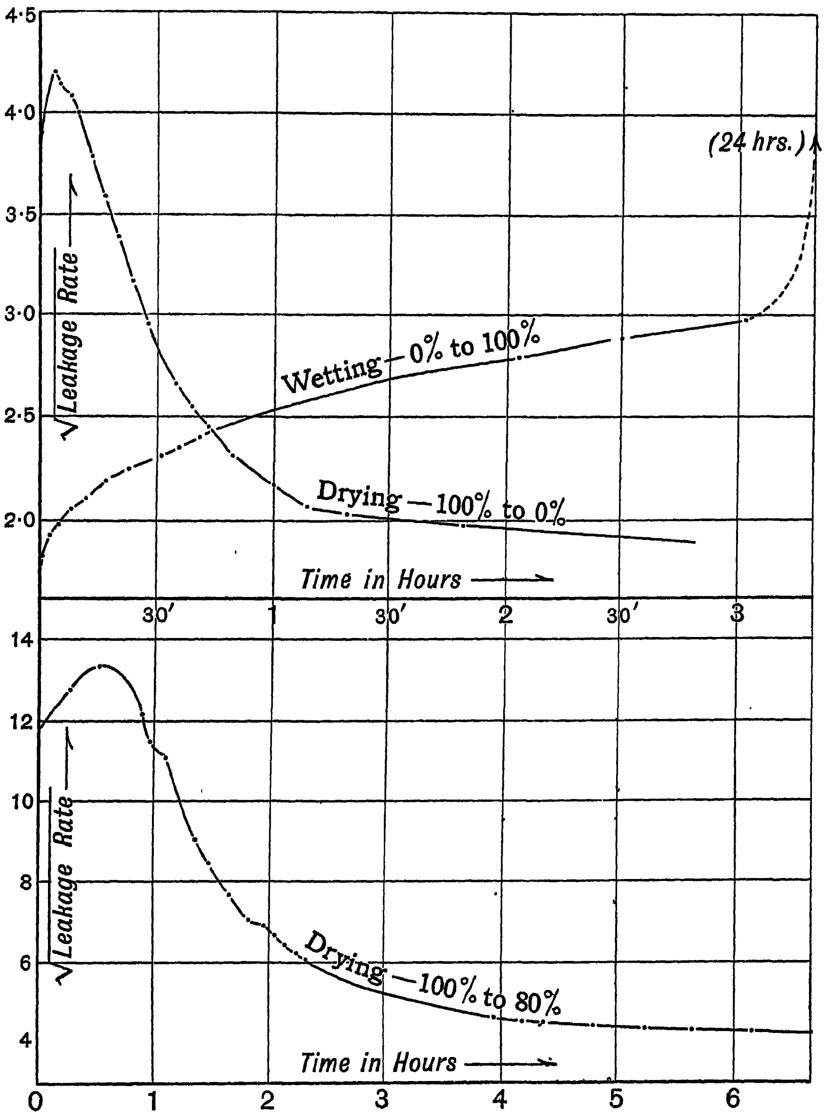

FIG. 14.—Changes of cross-section with lapse of time, as shown by the Porometer, after making an alteration in the relative humidity. The existence of inflections in the curves is clear. By using smaller changes of humidity, so as to traverse suspected phase-boundaries, these Time curves serve as useful detectors. Comparing the wetting and drying curves, water is given off more rapidly than it is taken up.

association between the water molecules played a part in their peculiar behaviour with cotton. Incidentally, these observations justified our use of Barr and Anderson's method for measuring cell dimensions, since the iso-refractive liquids used were shown thus to be inert to the cotton.

Turning to the other half of the system, it is interesting to notice that the latest work of Irvine indicates the formula $3(C_6H_{10}O_5)$ for the molecule of cellulose. The three groups are not in every respect identical, since one can be removed as glucose while the other two remain united as cellobiose. Hence it is not unlikely that if polar adsorption of water molecules were taking place, this might happen in at least two stages. All the available points for such adsorption on the first group having been occupied, two more water molecules (or groups of molecules) might be taken up at the two analogous points on the other two groups. Some such reason as this may quite possibly underlie the difference between some of the phases which we have found.

The molecular weight of the Irvine molecule being 486, while that of water is 18, the percentage of water taken up in these two steps of the simplest assumption would correspond to the water-contents of hairs near 40% and 80% relative humidity respectively. Some X-ray workers conclude that the cellulose formula contains four groups, with a molecular weight of 648, so there is nothing to be gained at present by elaborating this suggestion.

Moreover, any calculations of this kind are vitiated by our present uncertainty about the true dry weight of cellulose, which may have different values according to the severity of the drying process. It is not inconceivable that the water removed below 15% humidity is taken from the cellulose itself, by actual dehydration. Until these studies of the phase-boundaries have been carried far enough to identify the origin of the water in each phase, we have no basis for calculation. For the present it needs only to be noticed that there is some justification for expecting phase-boundaries to exist on elementary molecular considerations.

Some analogies to these phases may be seen in the hydration curve for salts containing water of crystallisation; good examples have been described with the Keen-Oden balance, and these also show clearly the statistical blurring of the phase boundaries as the size of the crystals is enlarged. The summation of various series of observations made by the late Dr. Williams, chiefly upon

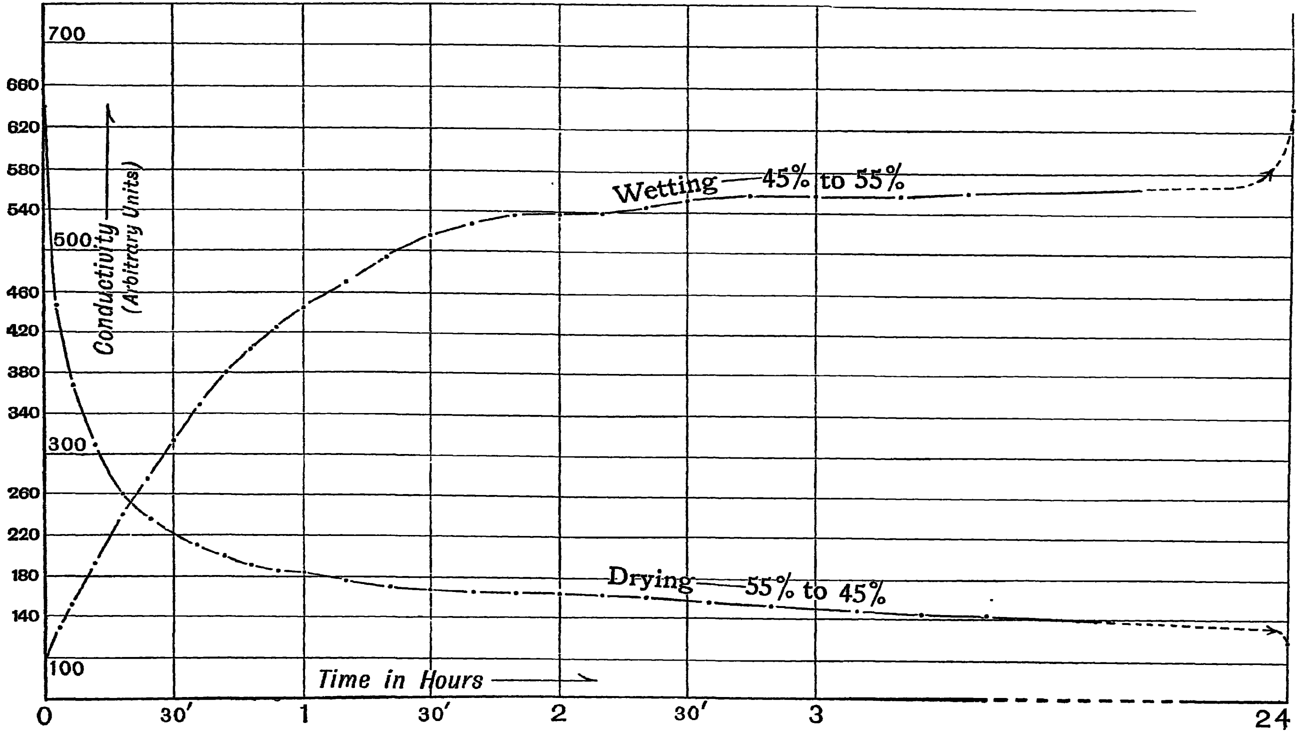

FIG. 15.—Similar Time curves to those in Fig. 14, but for electrical conductivity, and taken over a smaller range of humidity, inside a "phase." Again the water is given off more rapidly than it is taken up, and inflections are visible, *e.g.*, at 2 h. 20 m. on wetting.

(*Ibid.* Fig. 13.)

cotton boiled with soda, indicates that the adsorption rate changes suddenly at stages which correspond very roughly to our phase-boundaries.

The A-symmetry of the Hair.—We have previously had reason to think that the anisotropy of the microscopic structure is merely the gross consequence of molecular arrangements. For the detailed analysis of these molecular arrangements it would be most useful to trace the changes in each of the three dimensions of the hair, matching the weight of water taken up against the volume change —if any—in length, wall-thickness and wall-circumference separately. It is regrettable that no adequate methods for doing this are yet available; it would be more than interesting to find that all the swelling of the wall in some one phase took place in one dimension only.

We have made many measurements of these dimensions in various ways, as also have later workers. Our determinations of them at 0% and 100% relative humidity are fairly good, but the intermediate stages are unknown. The wall undoubtedly swells on wetting, and the percentage swelling appears to be unequal in the radial and the tangential directions. Some preliminary data suggest that the swelling is equal in these two directions if the results are calculated as radial expansion per growth-ring, and as tangential expansion per fibril. No importance need be attached to this result, which I quote merely to show how these a-symmetrical changes might be used when they can be measured with the same accuracy as the area or conductivity; if the result were true it would imply that water was taken up equally by the radial and tangential faces of the fibrils, and that the known visible structure of " domino " units did really match the form of the micellae.

Measurement of the longitudinal changes would seem easier to make, but even this is difficult to effect without introducing a continued longitudinal stress which vitiates the results obtained. No data worth discussion are therefore available.

A rough attempt was made to detect a-symmetry in the conductivity by looping two hairs over one another and testing all possible connections between the four ends; so far as the evidence went it indicated that the transverse resistance was about double the longitudinal resistance, as would be expected from the more intimate longitudinal bonds which we noted in the previous chapter.

Modifications of Cotton.—It is regrettable that previous workers have usually been very careful to purify their cotton; any kind of treatment makes some alteration in the delicate structure of the hair, thus exaggerating one phase at the expense of another, or obliterating some phases entirely. The familiar examples are such weight-curves as those given by special absorbent cotton and cellulose acetate; the curve for cotton which has been charred to a darkish brown is practically straight instead of being sigmoid. On the other hand, the weight-curve for carbon black more nearly resembles that of raw cotton, which illustrates the importance of investigating several physical properties simultaneously, not merely confining attention to one property.

The behaviour of cotton treated with strong caustic soda has many claims to interest, though it should be noted that ordinary mercerisation is a vague process; the soda only reaches a variable percentage of the hairs, so that observations should be made on hairs treated individually. The improved dyeing properties are presumed to be due to an increase in the suface available for adsorption of dyes, and the same will probably apply to water. The increased water absorption is rather dubious below 15% relative humidity, but from there onwards to the 77% boundary it is very distinct; above this, again, there is a suggestion of a change, giving a resemblance to raw cotton, as if the mechanism of water uptake in this phase had not been affected by the soda.

The greater amount of water taken up after mercerisation must be held by the cellulose in a different manner; although the hair is wetter at any given humidity, it is a worse conductor of electricity. Here, again, the determination of the dimensional changes is most desirable, for although the phase-boundaries do not seem to be shifted by mercerisation, yet the amount of tangential swelling would seem to have been reduced. Since the tensile strength of the hair is not appreciably affected, thus indicating that the soda does not react with the slip-surfaces of the fibrils, and since our preliminary results indicate that the percentage swelling of the wall-thickness is unchanged, it would seem likely that the soda acts on the radial faces of the micellæ or molecules. If the ratio of the dimensional changes in each direction were known accurately for both mercerised and raw cotton, we might be able to work out the relative and absolute dimensions of the micellæ.

Temperature.—All the characteristics here discussed have their temperature co-efficients, and no data of critical value can be obtained from cotton which is not maintained under fixed temperature conditions as well as fixed humidity. The Masson effect, whereby the cotton changes its temperature as a consequence of changes in humidity, also enters into the interpretation of the time-curves, and has incidentally explained [1] the sudden and enormous effect produced on the conductivity by the act of "degging" the floor below a refractory machine with a small amount of water.

A preliminary examination of the Masson effect was made without detecting any qualitative difference between the different phases.

Summarising the summary given in this chapter; it is evident that all the properties depend upon the physical structure of the cotton in such a way that their study can be used to infer that structure, where the observations can be made sufficiently exact. The cotton and the water may be combined in various phases, which we may designate quite provisionally as follows:

0–15%.	Phase of chemical hydration.
15–40%.	Phase of mono-polar adsorption.
40–77%.	Phase of tri-polar adsorption.
77–90%.	Phase of molecular association.
90–130%.	Phase of capillarity.

I have given these designations without regarding them seriously, but simply to focus the various converging rays of evidence, for the furtherance of subsequent studies on a wider base.

Our preliminary studies of the dimensional changes, though too untrustworthy to be detailed in this chapter, give me some reason to think that the micellæ, or cellulose aggregates, which are the constructional units of the living cell during its wall-building operation, do not contain less than seventy-two carbon atoms apiece, instead of the eighteen carbons of the cellulose molecule of Irvine. The minimum dimensions of the micelle would thus be of the order $10 \times 10 \times 30$ Angström units. Comparing this with the size of the "domino unit" of the previous

[1] By F. P. Slater.

chapter, which is 10,000 × 20,000 × 3,000 Angström units, it is obvious that we have a spacious region left open for further exploration.

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CHAPTER IV

THE SIZE AND VARIABILITY OF THE HAIR

The hair and the crop—Length—Cross-section—Weight—Strength—Stress.

The origin and structure of the hair having been described qualitatively, in an outline which I have made as extensive as our present knowledge will permit, the next step is to obtain some quantitative conceptions. For the purpose of the present chapter I propose to disregard the methods by which these properties of the hair are measured, devoting it merely to statements concerning the size of the hair, both relative and absolute. Further, since any raw cotton contains a great variety of hairs, it is equally important to note the kind of variation usually found in respect of each character, and to observe some of the causes which determine such variation. After doing so we shall be free to examine in the next chapter the technique whereby some of these measurements are made, and to restrict ourselves in so doing to those which Part II of this book will demonstrate to have some definite relation to the properties of yarn.

Much of the substance of the present chapter is contained in the reprint [1] of a popular lecture to a group of cotton operatives; it can therefore be somewhat condensed in its presentation here.

Taking the average data for the cotton crop of Egypt, the yield of seed-cotton per plant is about one ounce; after ginning this seed-cotton we obtain about one-third of an ounce of lint, or ten grams, the monetary value whereof is about one halfpenny. Just as spinning problems have to be considered in terms of the single hair, so problems of cotton-growing have to be analysed into those of the single plant, at a halfpenny each. It is interesting to notice that one plant will keep one fine-spinning spindle

[1] W. L. B., "Weights and Measures of the Cotton Hair," *Emp. C. G. Rev.*, 1925.

running for one day, the production of 120s yarn being at about this rate, after due allowance for waste.

The ten-gram yield of a single Egyptian cotton plant is spread over the ripening of some fifteen bolls, each containing about fifteen seeds. The weight of lint per seed is thus in the neighbourhood of 0·04 gram. The number of hairs on a single seed may be taken as ten thousand. Thus the total number of hairs in the crop from a single plant is round about three millions.

By thus presenting the operation of cotton spinning as a process which endeavours to lay out three million hairs in orderly sequence each day for each spindle in the mill, the desirability (p. xix) of stating the process in single-hair terms is emphasised.

Hair Length.—The range of "length" in the world's cotton is between half an inch and two and half inches. This statement refers only to the length of the longer hairs, and there is a great deal of confusion in the meaning attached to statements about length. Length may be expressed as that of individual hairs, as the length found in a representative group of hairs thus measured, as the mean length of such a group, or as the commonest length ("modal length"). It is commonly expressed as "length of staple," which has various meanings, but generally lies midway between the modal and maximum lengths. Measurements made while the hairs are in position on the seed, after they have been combed out into a halo, are surprisingly exact in giving expression to the greatest length found abundantly on that seed; this arbitrary measure is called the "halo length." We need only consider the expression of length by the measurement of individual hairs, as effected with the Sorter or by hand; in the latter case we can only deal with a few hairs, and most elaborate precautions to ensure correct sampling are necessary.

Thus it happened that after twelve years' constant familiarity with cotton in the field and the laboratory, I was utterly astounded at the variation in length shown by two samples of short-staple cotton which were the first to pass through my newly-invented sorting device. Later sortings of Sea Islands seemed even more incredible; hairs were present which were only half an inch long, longer ones became more common up to the modal length around an inch and a half; then longer ones became less common until the maximum length was reached beyond two inches, though the reputed staple length was less than two inches.

This smooth variation of length cut across all preconceived ideas as to the reality of length-estimations. It is now commonplace knowledge, but it was almost unbelievable at the time.

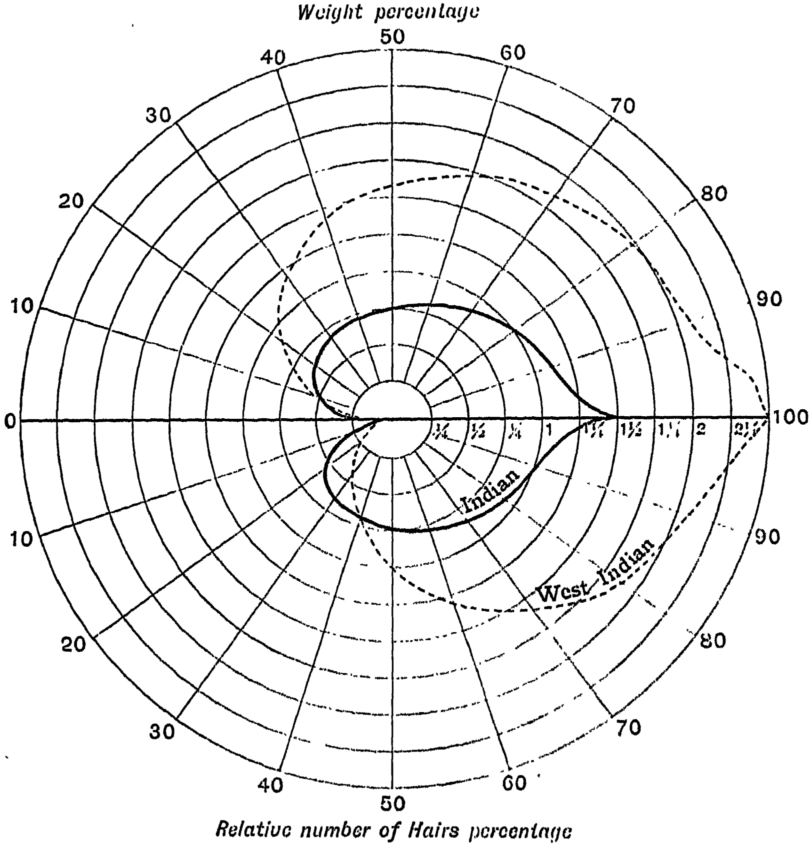

FIG. 16.—Polar co-ordinate plotting of hair-length data from the sorting of cotton, to simulate the "halo" of combed seed-cotton. The curves are not frequencies, but arrays, summing the total percentage of hairs attained at each step of increase in length. Distortion due to the "hair-displacement error" is manifest in the pointed butt of the ovoid halo.

The data are the same as those in Figs. 21–22, the arrays for Indian and Sea Island cottons being plotted by weight-percentage in the upper semicircle; the number-percentage arrays, as obtained in that Fig., being presented in the lower semi-circle.

The subsequent accumulation of data from cottons of all kinds, grown in all parts of the world, has also eliminated another misconception and has provided a general conclusion of some interest. At

the first glance it seemed that the short cottons, such as the Asiatics, were more uniform in length than the long ones, but this was simply because the former extend over a smaller range, and are liable to be subdivided into fewer length-groups. The conversion of the frequency curve to a generalised form, independent of absolute length, can be done by plotting the data as an " array " on semi-logarithm paper and subdividing this array at fixed intervals ; or,

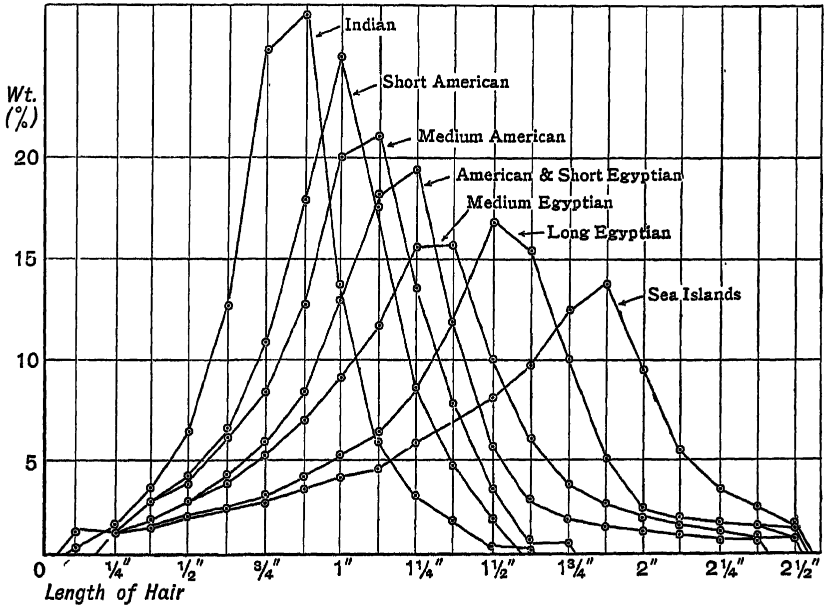

FIG. 17.—A representative set of Sorter curves, for hair-length frequency distribution by weight-percentage, classified in one-eighth-inch groups. About ten samples of similar modal length are averaged in each curve. That the general distribution would be similar if each sample were divided into the same number of classes can be seen from the nature of the change in modal percentage.

a rough comparison can be made directly if the array is plotted on polar co-ordinate paper, thus simulating the halo of a combed seed. If this generalised curve be obtained for the average of several samples of Indian, American, Egyptian or Sea Islands, it is found that all are alike ; the usual relative variation of hair length is similar throughout the genus.

This conclusion seems reasonable if we remember that the hairs are a part of the seed-coat, and that the form of the cotton-seed is always much the same. If the hairs stuck out straight like

quills on a porcupine, I think we should expect similar resemblances of form in their arrangement also. It is at any rate a conclusion of practical importance, for it enables us to define a "normal sorter-curve," which at once provides a means of defining real standards of staple length for use in arbitration and in recognising and measuring deviations from type. This normal curve appears to follow a logarithmic rise from zero to the modal length, followed by an arithmetic fall to the maximum.

Just as differences exist between the shapes of the seeds borne by different varieties, some being fat and some thin, in spite of their general conformity to the average shape of all cotton seeds, so we have differences between the length-variations in different cottons, and each variety has its own characteristic sorter-curve which deviates more or less widely from the general type. The most uniform length known to me was given by my pure strain No. 111, while at the other extreme it is not difficult to imagine cottons in which all lengths were equally common. The existence of these varietal peculiarities does not, however, invalidate the existence of our "normal sorter-curve."

On attempting to define the characteristic sorter-curve for any variety we meet with a difficulty which is of general interest. The halo-length, or commonest maximum length, varies from seed to seed; thus the sorter-curve made from the lint off a number of seeds is a compound construction. As an extreme case, let a mixture be made of the lint from a Sea Island seed with that from an Indian seed; the sorting of this mixture will give a curve with two distinct modes, say at $\frac{3}{4}''$ and at $1\frac{1}{2}''$ respectively. Obviously then, to appreciate the sorter-curve for any given crop we must analyse it into the curves given by each constituent seed. On doing this we find that for any given variety the form of the curve is the same for each seed, if we eliminate the variations in halo-length as we eliminated them to find the "normal sorter-curve" for all cottons. But this is only a re-statement of a fact familiar to the plant-breeder, namely, that each variety has a characteristic halo when its seed-cotton is combed. It was not practicable to express the shape of this halo quantitatively until the Sorter became available.

The clear-cut form of the curve obtained from single seeds of a variety becomes necessarily blunted and blurred when millions of seeds are taken together, as is the case in field crop. While

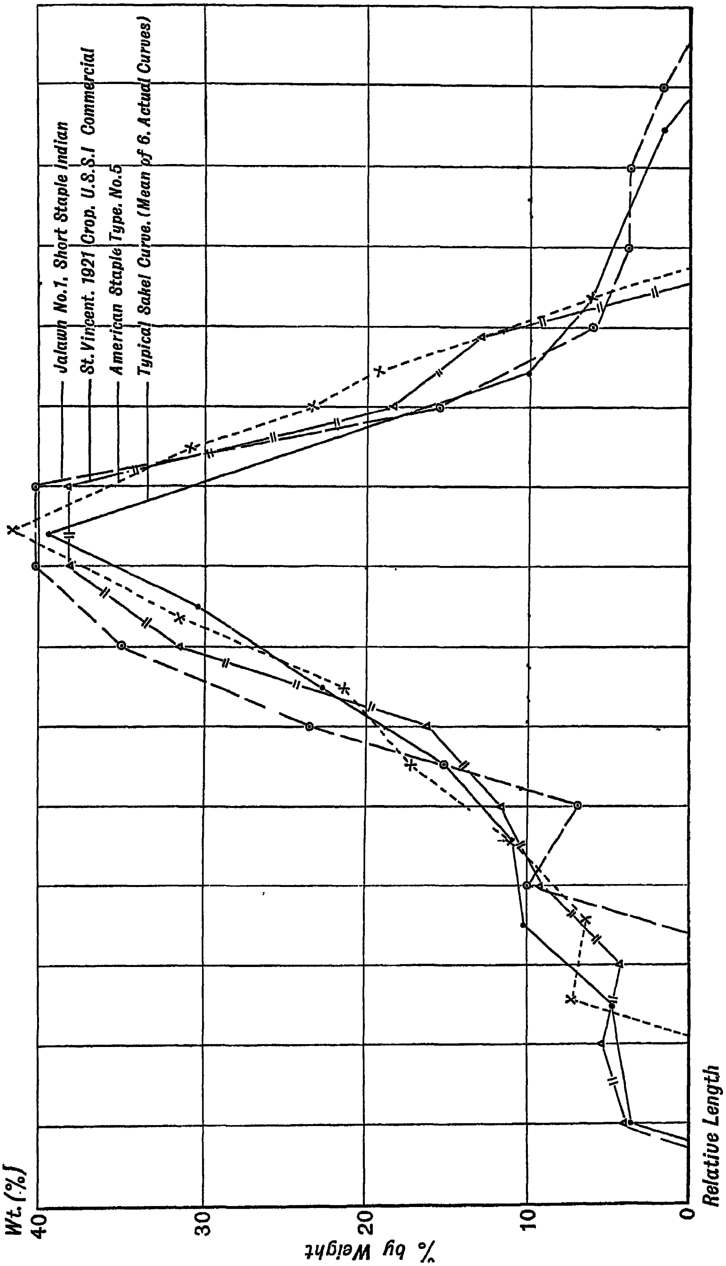

FIG. 18.—Comparison of the *relative* length-distribution in single samples of Indian, American, and Sea Island cottons, with that of the average for several samples of Sakel, thus showing substantial identity of *relative* length-variation in these very dissimilar types.

this original form would seem to be fairly independent of the environment, the position of its mode (or maximum) varies from seed to seed under environmental influences, as I showed in *Raw Cotton*. The form of the curve given by the total crop thus depends partly on the genetic form given by the single seed, and partly on the relative proportions of seeds of different length which the environment has determined. How this compound curve varies, even from day to day, can be seen in the contour-diagram at the end of my Handbook on the Sorter.[1]

I shall not attempt to discuss the cause of the fundamental variation from one part to another of the same seed. No doubt it is largely a question of nutrition, dependent on the anatomy of the vascular bundles in the seed-coat. Though there is much more research to be done in this direction, it would seem that the length on any one part of the coat is fairly uniform.

Cross-Section of the Hair.—The length of the hair is a fairly obvious thing. It is probably because of its obviousness that it has been given so much importance in the past. The other measurable characters are not open to determination by the unaided hand and eye, though they are all more important than mere length, so far as the evidence I have collected in this book can show.

The cross-section of the hair shows every possible variation of shape when the wall is thin, and is circular when the wall is very thick, but is usually ellipsoid, as the natural consequence of the collapse of the tube. The major axis of this ellipsoid is the dimension to which I have formerly given the name of "ribbon-width," in order to avoid ambiguity and confusion with the word "diameter," which should properly be restricted to the true diameter of the cell before the tube collapses.

On the average of various samples of the world's cottons we find that such an extremely coarse one as Burmese Wagyi has a major axis of 27 microns (0·027 mm.) with a minor axis of 20 microns; the latter dimension is better stated as a wall-thickness of 10 microns. A very fine Sea Island replaces these figures by 15, 10 and 5 microns respectively. The cross-sectional areas corresponding to these dimensions are roughly 450 square microns for the coarsest cottons grown, and 130 for the finest.

The variation from hair to hair upon a single seed is greater

[1] *Loc. cit.*, p. 44, Fig. 8.

than that shown by the whole range of the cultivated cottons of the world. Areas which vary from 50 to 500 square microns are typical.

The proximate causes of this variation can be traced from the life-history of the hair. It depends only slightly upon variations in the original diameter of the cell. Principally it is brought about by differences in the thickening of the secondary wall, already described.

Hair Weight.—The weight of a hair will necessarily vary with its length, but if the latter is eliminated by expressing the weight as that of a unit length, which is conveniently one centimetre, we then find that the "hair weight per centimetre" ranges from 0·0036 milligram to 0·0009 milligram in passing from Wagyi to the finest Sea Island. This range is of the same order as that for cross-sectional area, showing that the density of the cell-wall is roughly similar for all the world's cottons. Whether this similarity holds good in detail for individual kinds of cotton is still rather uncertain.

The range of variation of hair weight between single hairs of one and the same kind cannot yet be determined by direct weighing, but it is evidently indicated with sufficient clearness by the variation in cross-sectional area. Hairs which weigh only 0·0005 milligram per centimetre must be common in any sample, while the maximum will depend on the cross-section.

Hair Strength.—The breaking load of the hair under standard conditions of loading is necessarily also affected by the cross-sectional area. The exact measurement of the latter is much more difficult than the determination of hair weight per centimetre; consequently, while the breaking load should be expressed in terms of "stress per unit area of cross-section," it is actually far more convenient to state it as "breaking load for some standard arbitrary hair weight."

This breaking load for equal hair weight may range as widely as from 8 to 20, using arbitrary units of expression. Moreover, the kinds of cotton which have the intrinsically weaker walls are also frequently of coarser cross-section. Thus the range of the world's cottons when measured by the actual average breaking load of the hairs shows a smaller range than that of the hair weight, *e.g.* 9 to 16, instead of 9 to 36.

The breaks of individual hairs again range even more widely.

A group of a hundred hairs with a mean break of $5\frac{1}{2}$ grams, which vary individually between 1 and 10 grams, is typical for good Egyptian cotton. In such a cotton as Rough Peruvian the range is very much wider than this.

The strength of the primary wall, which we have seen to be the main constituent of "neps" (p. 19 and 114), is much less than that of the secondary wall. The relation can be traced by testing the hairs from bolls which have been artificially opened at various stages of wall-building. Some field material of this kind was collected for me in the Sudan by Mr. R. E. Massey, and the breaking load was found to increase as the number of growth-rings increased, these numbering about 35 in the mature hair, thus:

Components of the cell-wall.	Break in grams.	Weight per cm. in mg.
Primary wall (and protoplasm) on 35th day .	1·00	0·00050
Additional growth-rings; average for each one .	0·14	0·00003
Total wall on 70th day	6·00	0·00150
Secondary wall only, by difference . . .	5·00	0·00100

Hence the "break per 0·001 mg. of hair weight per cm." in these observations is:

For the primary wall . . 2 grams.
For the secondary wall . 5 grams.

True Breaking Stress.—While it is not practicable to calculate this stress for everyday use, it is easy to approximate from the data given above. In doing so it should be remembered that the cell-wall has the spongy structure already described, so that although the density ascertained by measuring the cross-sectional area is not much above 1·00 specific gravity, yet that of the solid cellulose is 1·55 specific gravity. That this latter accepted value is correct may be roughly demonstrated by measuring the displacement of alcohol, which easily penetrates the wall.

Making suitable allowance for this spongy structure, the tensile breaking stress of the fibrils in the worst of the world's cottons is about 8 tons per square inch, while the best reach 20 tons per square inch. These values are comparable with those for copper and for mild steel respectively.

The Reversing Spiral.—The reversals of the spiral may take

place at intervals of only one-hundreth of a millimetre, or even less. In the other direction the longest interval between two reversals which has been recorded is 12·7 millimetres. The average distance is in the neighbourhood of half a millimetre. An ordinary short-staple hair thus usually contains some fifty reversals of the spiral. The frequency curve which shows the variation of the intervals between reversals is characteristically skewed, so that

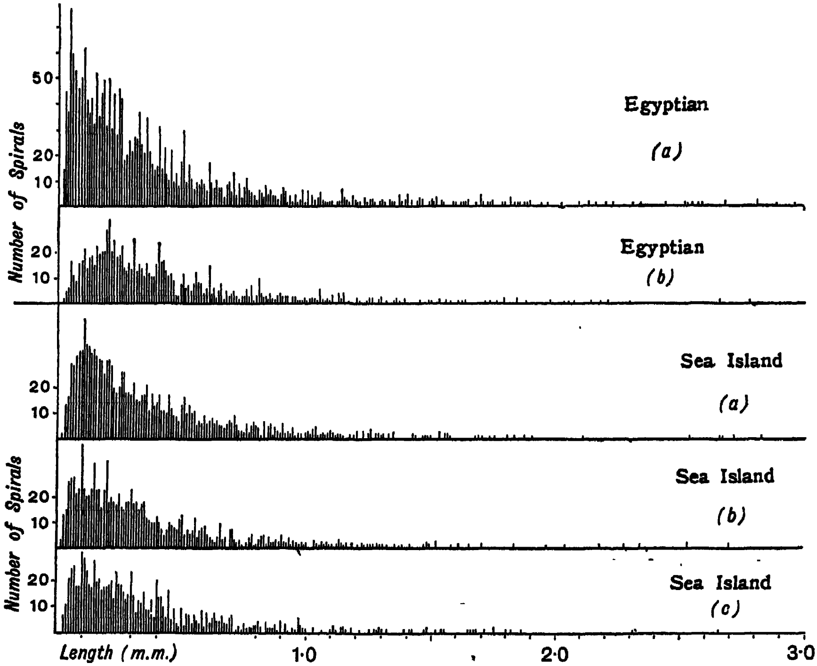

FIG. 19.—Showing the variation in the length which separates two reversals. Measured above on twenty hairs which were 40 mm. long, and below upon twenty hairs which were only 20 mm. long. All the hairs were taken from the same seed.

the commonest interval is much less than the average interval; this commonest (or modal) interval is in the neighbourhood of one-tenth of a millimetre only.

Convolutions.—The size of the convolutions is affected by the wall thickness, as I have already noted in *Raw Cotton.* It is also influenced by the occurrence of reversals. The average of this very variable characteristic may be taken roughly as five convolutions per millimetre, or 200 microns per convolution. Taking

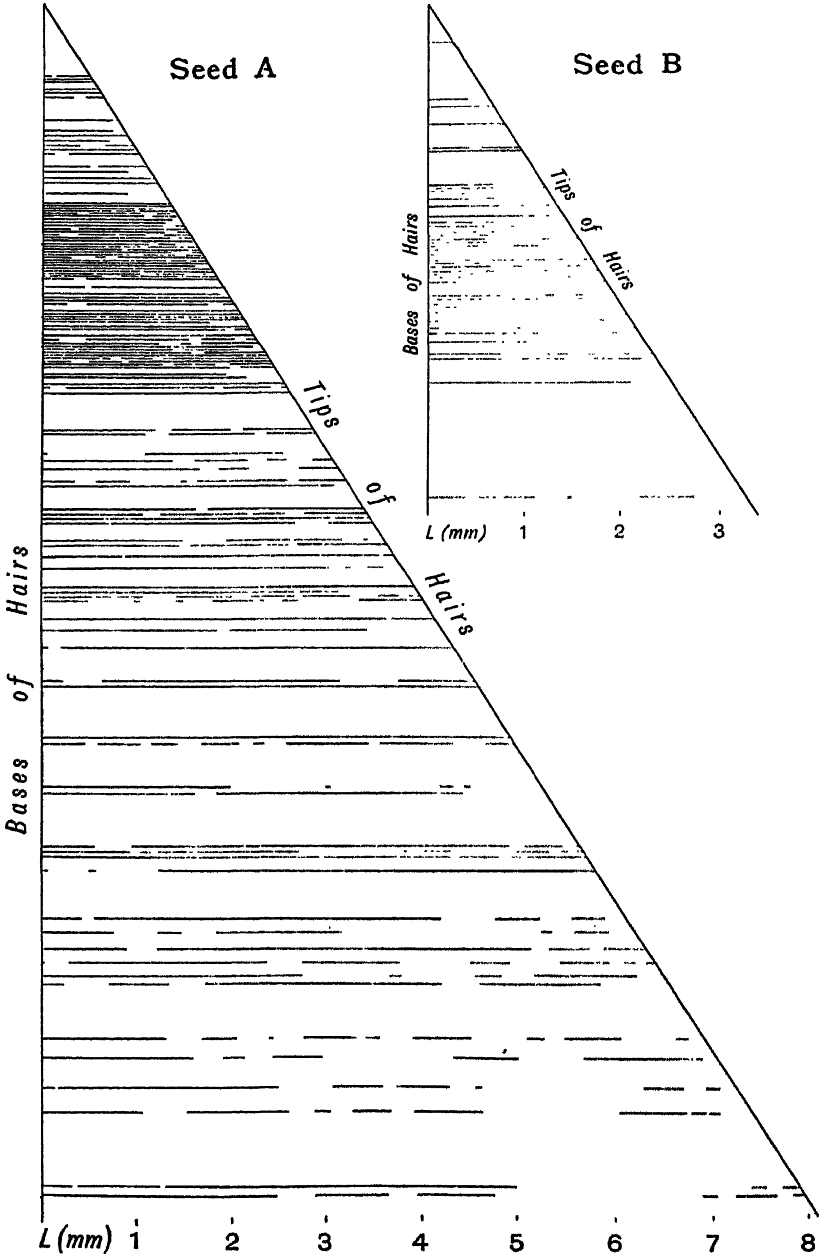

FIG. 20.—Showing the variation in reversal arrangement found even in short fuzz hairs, less than 8 mm. long. The hairs are arranged in order of their length. Sinister spirals shown by black lines, dexter spiral left blank. The 6% of hairs which had dexter basal spirals are omitted.

20 microns as an ordinary value for ribbon width, it follows that the average convolution is about ten times as long as it is wide. The modal or commonest form is again shorter than this.

Fuller details concerning the variability of the two last characteristics will be found in a series of papers already published.[1] With respect to the characteristics which are dependent on the amount of secondary wall laid down—cross-section, weight and breaking load—the same general considerations apply as for the length of staple, but with additional opportunity for fluctuation from hair to hair; to discuss the opportunities in detail would take us too far into some rather speculative regions of plant physiology. Detailed records for the variability of most of these characters in a group of American cotton samples will be found in the later chapter (Chap. XIII) which presents the observations on " The Thirteen Samples."

Meanwhile the reader should now have sufficiently definite ideas of the magnitudes involved in hair measurements to pass on to the actual processes involved in making such measurements.

[1] *Vide* General References to Chap. II.

CHAPTER V

HAIR TESTS

Sampling—Length—Ribbon, wall and sectional area—Weight—Break—Convolutions—Slipperiness—Minor tests.

ALTHOUGH even an up-to-date Testing Laboratory is as much concerned with yarn (Chap. XII) as with the raw cotton, yet I propose now to deal with the latter side of its work independently. This will conclude our examination of the cotton hair by noticing how the information is obtained which we shall utilise when discussing its relations with the yarn. Such separate treatment of the hair tests should also be useful, because the whole subject is novel—or was so when my methods were first worked out—and I have already given reasons (Chap. I) for believing that it will become an important aspect of cotton technology, as a supplement to the grader's judgments.

The reader's attention was directed in the Introduction to the important part which the provision of suitable methods has played throughout these researches, and this is especially so in the testing of hair properties. The methods here described have the distinction of originality, and they have been in use long enough to ensure their suitability, but they are not described from any conviction that they are the best methods possible; indeed, I have been at some pains to point out their defects and difficulties, in the hope that other students will be thereby assisted to improve upon them.

The broad general distinction between Testing, which provides numerical data for statistical research, and Experiment proper, which may provide exactly the same data, lies in the importance assumed by the time-factor when "tests" are being made. The methods of the testing laboratory must be so standardised that the desired information can be obtained quickly, and without involving any appreciable error due to the personality of the individual observer. Thus it may happen that a method of

measurement is devised in the first instance solely for the purpose of obtaining the desired facts, without any regard to the time or trouble required. If it should appear afterwards that such facts would supply useful routine information, there must be an overhaul of the experimental method; it is studied with the stopwatch in order to find out how and where the valuable time is consumed; parts of the apparatus are rearranged in consequence, so that manipulation may be speeded up; all possible adjustments which might accidentally alter the scale or zero of the readings are eliminated, and automatic manipulation is devised where such is seen to be possible. Automatism is the chief aim of every testing device, because it avoids the personal factor entirely; the errors of a machine are more predictable than those of a human being. The time-factor is also affected directly by automatic working, because such working cuts down the time during which active attention by the operator is required.

The available equipment for testing cotton hairs is as yet so imperfect as to show every stage of this evolution, passing back into methods which cannot yet be speeded up sufficiently to permit of their use in routine testing. The chief difficulty is the manipulation of the individual hairs, for which there is no substitute except the fingers and forceps, except in the measurement of length by the Sorter. That there should be any exception is reassuring, for it may be taken to imply that in the future we shall be able to count, weigh, calliper and bend the hairs as easily as we now measure their length; at present I have not seen how to do so, though length-measurement is intrinsically no more difficult than the others.

Sampling.—Sampling is only a preliminary to the actual testing, yet its importance is such as can hardly be over-emphasised. I have no doubt that many of the errors of judgment made in grading could be traced back to inadequate sampling. The standard practice of the spinning mill in its elaborate routine of the mixing-room is only an indirect tribute to the heterogeneity of cotton, and to the consequent importance of sampling. Having already followed the developmental history of the hair in its relation to the bulk crop (pp. 44–50), my reader will be able to realise that even the skilled blending practised in the re-baling of Egyptian cotton cannot make a bale so homogeneous that a single sample from some part of it may be taken as a "true

sample " of the whole bale. When we make actual tests of such single samples, by methods of sufficient accuracy, we can detect discrepancies from one part of the bale to another which make it quite futile to attempt the testing of hairs unless we can be confident that our samples are really true ones.

The number of hairs tested is dependent upon the character examined, but the hair-to-hair variability of cotton is so great that reasonably dependable results cannot be obtained on less than a hundred hairs even for the easiest characters. In such a character as the breaking-load, which is itself the resultant of several characters (cross-section, breaking stress and irregularities of profile), an average value of 100 will include individual hairs in that average which range from near zero to 200; here the " probable error " for one hair is 30%, so that the average value obtained from a hundred hairs may range between 90 and 110 by the pure accident of sampling. The number of hairs tested must therefore be raised above a hundred when complex inter-relationships are under examination, but for many purposes this number is just sufficient. These hundred hairs must themselves be a true sample of the sample which we have drawn from the bale, lap, sliver, roving or yarn.

When hairs are to be taken from yarn it is often necessary to untwist a long length; this can be done automatically by an " Unspinner " (see Appendix). Otherwise the cotton is in a fairly convenient form for sampling. When sampling from cotton which has passed the finishing draw-frame in the mill, no sampling precautions are necessary, because the mixing which has been effected at this stage is so thorough, as we shall see when discussing the drafting-process (Chap. VIII). When the cotton is drawn at any earlier stage, whether from boll, gin, bale, lap or can, a routine procedure of subdivision must be followed.

Our object is to obtain a true sample which weighs about half a gram, this being about the weight of the nine-inch length of sliver used in the Sorter. By working to this size it becomes practicable and convenient to make use of the little draw-box employed in preparing the sliver for sorting,[1] and thus to effect very perfect admixture of the hairs within this sliver; hairs may then be taken from any part of the sliver at random for

[1] W. L. B., *A Method for Measuring the Length of Cotton Hairs*, Macmillan, London, 1921, Chap. III, and Fig. 9.

individual tests. To require that this half-gram shall be itself a true sample of a whole bale of cotton would seem at first sight to be preposterous, but the absurdity disappears if we remember that this small weight is composed of some fifty thousand separate hairs, for we are effectively taking fifty thousand samples from the bale.

It is true that we cannot do this literally. If we attempt to pick out individual hairs we no longer obtain a true sample; the hairs most easily noticed, gripped and removed are the long, stiff and heavy ones. The same unconscious selection will apply to a less degree if we drag out small tufts of hair, though the selection will then give us merely an excess of long hairs, for self-evident reasons. Consequently, we must use some method of subdivision, so that we always retain the exact equivalent of that which we reject.

Such subdivision is ideally effected by taking the whole bulk of the cotton to be sampled, and dividing it into equal halves, then halving these again, and so continuing until thirty-two equal portions are obtained. From each of these we then take a sample which is not so small as to run the risk of unconscious selection, already mentioned, and these thirty-two samples have then to be recombined. The recombined material would be much larger than we need for making a sorter sliver, so that each of these thirty-two should be halved and re-halved individually, until it is reduced to about one-tenth of a gram. The recombination is then made from these fractions, and so gives us about a gram and a half of cotton, which must be thoroughly mixed before any of it is thrown away in reducing it to the half-gram required finally.

This admixture is done by making it up by hand into a rough sliver, and then passing this sliver through the draw-box of the Sorter. By doing so at least a dozen times, four-folding the emergent sliver after each passage, and taking care to use the maximum practicable roller setting together with the least possible pressure on the back roller (to avoid hair-breakage), the sliver becomes homogeneous, and may then be cut down to the size and weight required.

I have said above that the subdivision and recombination is "ideally" effected in this way, because it is obvious that practical limitations must often prevent us from actually halving and

quartering the mass which we are sampling. But the reader should be careful not to confuse limitations of necessity with mere limitations of convenience. Attention to the details of sampling is not mere scientific fussiness, but is of fundamental importance to any kind of testing, and is especially important where hair tests of cotton are involved, because raw cotton is one of the most difficult materials with which the would-be sampler can be confronted. Those difficulties can only be evaded by the method of symmetrical subdivision or repeated halving.

Nor should it be imagined that the same care need not be applied when the original bulk of material is small. I have known lack of care in this respect to cause the breakdown of observations made on provided samples which only weighed an ounce or two. Some of them had been made up from a mixture of two different cottons, the admixture had been made without these statistical precautions which I have just described, and the two ends of the little packets gave different results.

I would therefore repeat the previous assertion, before proceeding to the actual details of hair-testing—that the importance of careful attention to every detail in the sampling of cotton can scarcely be too strongly emphasised.

Length.—The Sorter mechanism (p. 361) was re-designed as the Sledge pattern in order to provide a suitable and safe form of the instrument for use in the testing laboratory, and this is the most important method available for measurement of length. We have already seen (p. 10) that any information about length which does not provide detailed information about the length of every single hair, as in the form of a frequency diagram, is relatively of little use and may even be misleading.

It may occasionally be necessary for special purposes to measure the length of individual hairs by hand. Some trouble has been taken elsewhere to do this with great exactitude; thus the hair image has been projected upon a screen and measured by running a map-measurer over the picture.[1] However, the variability from hair to hair is so great that even if the limited number of hairs which can thus be measured is a true sample, measurements made in this way are worth no more than if they had been made by laying the hairs on a piece of one-way black

[1] N. A. Cobb, *loc. cit.*

plush and measuring them quickly with dividing compasses transferred to a millimetre scale. The statistical consequences of the variability of length, as of other characteristics also, do not seem always to be realised; thus, the temperature and humidity at which staple length must be measured are defined in the U.S.A. by law, but in order to arrive at a degree of accuracy which would detect changes in length due to any likely variation of atmospheric conditions it would be necessary to average the individual measurements of at least a thousand hairs; having first ensured that these were a true sample, that is to say, and quite apart from the ambiguities which we have seen to exist when length is expressed in any other way than by single-hair frequency measurement.

The one-way plush just mentioned is one of the most important items in the equipment of a cotton testing laboratory. I owe the first suggestion of its use to Mr. McConnel, and the provision of the actual material to Messrs. Lister of Bradford. The pile is laid over in one direction, and will straighten and hold any hairs drawn along the surface against this slope, while it can instantly be cleaned from adherent hairs by rubbing in the same direction as the slope. Moreover, if the plush is orientated below a strong light, so that the pile points in the right direction, it provides the most nearly perfect dead-black background obtainable; against this background the hairs are conspicuous objects by the contrast of their brilliant reflections. The reduction of eye-strain by this device is a very important factor in single-hair manipulation.

Turning to the measurements of length by means of the Sledge Sorter, we have the advantage that many thousand hairs are measured individually in about three minutes' manipulation of the instrument. As the sledge is rolled along a strip of calibrated black plush, it leaves behind it a deposit of hairs, these having been left on the plush individually in positions corresponding to their respective lengths. The composition of the original sliver can be seen at a glance by mere inspection of the plush, but for purposes of exact record it is necessary to sweep up the deposit into convenient groups, weigh each little heap on a simple torsional micro-balance, and plot from the data a curve showing the variation as percentages of the total weight between the various length-groups selected. These groups are con-

veniently taken as successive eighths of an inch, so that, *e.g.*, the one-inch group contains those hairs which were swept up between the $\frac{15}{16}''$ and the $1\frac{1}{16}''$ calibrations of the plush.

The sliver used for sorting is nine inches long, this length being fixed by the circumference of the folding roller in the draw-box. It should not weigh more than half a gram, and may have to be less when very fine long cottons are being examined. With the greater weight it is not necessary to take more than twenty runs of the sledge along the plush, and even when many more

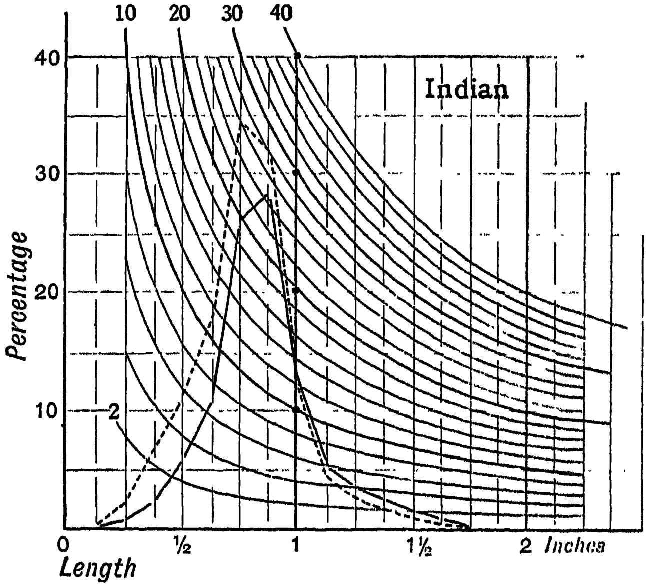

FIG. 21.—*See* FIG. 22.

runs are employed it is still the case that the time spent in this actual test is only a small fraction of the whole operation. I mention this because it has frequently been suggested to me that the movement of the sledge or of the plush might be made automatic, to save time; in point of fact, I deliberately eliminated the automatic action in this respect, so as to make the appliance portable, and to ensure that any abnormal behaviour of the sliver would at once be noticed by the operator. The predecessor of the sledge pattern, as well as its successors, were all automatic.

The time required for the test is only two or three minutes, if a sliver is already available, and if mere inspection of the plush is all that is needed. If quantitative results are required, as is always the case in laboratory routine, another quarter of an hour is required at least, for weighing, computation and plotting of the final sorter-curve. If the sliver has to be made from a sample of raw cotton the whole test will take upwards of half an

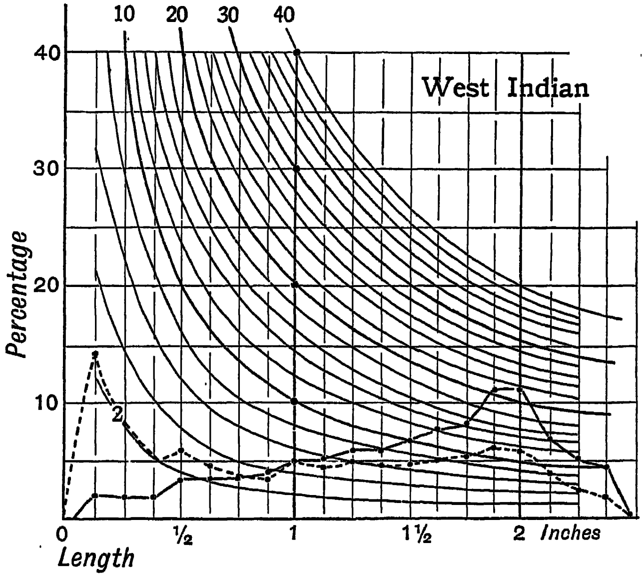

FIG. 22.—Nomograph paper for plotting data obtained with the sorter. The usual percentage distribution by weight is plotted as usual to the rectangular co-ordinates. If the same data are then plotted again, but this time using the curved abscissæ, the resulting frequency distribution can be read off on the rectangular co-ordinates as being the relative distribution by number of hairs. This makes the sufficiently valid assumption (Fig. 34) that hair-weight is independent of hair length.

Two extreme types of cotton are thus plotted, after sorting to the same length-classes. The flat-topped Number curve of the Sea Island is statistically interesting, and there is evidence of hairs broken accidentally at its short end.

hour, the time required for making the sliver being dependent on the skill of the operator and on the perversity of the particular cotton.

It is debatable whether the results are best expressed by percentage weight or percentage number. The weight-curve looks more useful industrially, and has the advantage of being

obtainable directly from several thousand hairs in a short while. The actual numbers are more important for such purpose as the study of drafting, but the counting of hairs in simple and obvious modifications of the instrument is a tedious task. Fortunately the differences in average hair weight per centimetre between hairs of various lengths are not important, as we shall see later (Chap. IX), so that the weight-frequency is easily converted into the number-frequency. Calculation of the conversion may be avoided altogether by a simple nomograph.

There are certain sources of error in the use of the Sledge Sorter which are more conveniently dealt with later (p. 135). I mention them here in order to point out that these errors are not serious, and that the scientific caution of my notes on the subject in its Handbook has been taken too seriously by some novices. I wrote that "false results can be obtained by careless handling," but much less care and no more skill than are given to adding up ledger columns or slowly typing a letter will ensure identical results with those obtained by other persons using other Sledge Sorters on the other side of the world on the same cotton.

Ribbon-Width, Wall-Thickness and Sectional Area.—It is convenient to consider these measurements together. They are not of outstanding importance in routine testing, because similar information of much greater exactitude can be obtained from measurements of the hair weight.

The form of the cross-section of the hair may be widely different from the type-form, which is that of a symmetrical tube in collapse, with the internal faces of opposite sides in contact. Consequently there is an appreciable error involved in calculating the cross-section from measurements of ribbon and wall, which must be assumed to have been made on typical hairs. The direct measurement of area from micro-photographs or camera lucida drawings of hair sections which have been cut with a microtome is also untrustworthy, on account of the distortion which the razor may have produced, even if every hair is strictly perpendicular to the plane of the cut; the magnitude of this distortion must be erratic and cannot be assessed.

The cutting of sections is easily and rapidly done by double embedding of the hairs, first into hot celloidin, and thence with a minimum of adherent celloidin into hard paraffin. A simple microtome with hand feed and a large heavy blade set at a high

angle is the most suitable instrument. No methods which involve preliminary softening of the cellulose can be permitted.

In the ordinary routine of the testing laboratory it is only practicable to make those measurements upon undisturbed hairs, viewing in profile under the microscope, and measuring the ribbon and wall in at least a hundred hairs, preferably at one place only, near the middle of each hair. Such measurements can, of course, be extended, but their present importance is not sufficient to warrant more time being spent upon them.

The ribbon width by itself can be measured with reasonable accuracy in this way, the hair being mounted in water to fix the standard of humidity, and a micrometer eyepiece-scale employed.

Measurement of the wall-thickness is a very different matter, and—as we have seen when examining the moisture relationships in Chap. III—no really accurate method is available. If the wall-thickness be measured when the hair is simply mounted in water, it appears to be nearly double the true thickness on account of the refraction of light in passing from water to wall and out again. Also, the hair is doubly refracting, thus preventing sharp definition of the edge of the central canal, at the inner boundary of the wall; this can be prettily demonstrated by mounting a hair over the graduated scale of a stage micrometer, when the graduations seen through the hair are duplicated.

For the measurement of wall-thickness in conjunction with ribbon width the hairs must be examined in a liquid of the same refractive index as the hair, by a modification of Barr and Anderson's method (Appendix), and only one of the two rays employed. The width of the central canal being thus measured, it is subtracted from the ribbon width, and the difference is halved to give the wall-thickness.

To convert these two measurements into cross-sectional area we must employ an empirical formula; the least objectionable of these was ascertained by comparing such measurements with the similar measurements made on cut sections of the same cottons, and with the known area of these sections. This least objectionable formula assumes the cross-section to be two semicircles connected by a rectangle, and is not likely to be more than 5% in error. The actual formula is: $2w(r - 2w) + \pi w^2$. It is sufficient for the ordinary comparison of similar cottons when such comparison is needed, until further research has demonstrated

the existence of connections between hair shape and the density of the yarn, such as are hinted at by some of the facts now available. It will then be necessary to take the microscope measurements more seriously.

The original diameter of the uncollapsed cell can also be inferred approximately from the same measurements, which, on the same assumption of two connected semicircles, will give the circumference of the collapsed cell, and hence its uncollapsed diameter. The same value can be obtained independently, though again approximately, by Dr. Harland's device [1] of measuring the "ribbon width" of hairs which have been swollen with caustic soda.

Hair Weight.—The unsatisfactory nature of the technique for measuring "fineness by volume," as discussed in the preceding section, is fortunately immaterial, because we have an alternative method for measuring fineness by weight. This latter is by far the most accurate test which can yet be made upon cotton hairs, besides being the quickest, and giving the most important result.

So far as I am aware, the measurement of hair-weight was not employed until I introduced it in order to estimate the number of hairs on the seed, in the course of my studies of ginning-out-turn. The passage of time has shown it to be the fundamental industrial characteristic of cotton, far more important than the length of the hair. When it is used in conjunction with measurements of the breaking load of the hair it is competent to account for the major part of the properties exhibited by any given yarn.

I have always used the term "hair weight" for convenience, but it should be repeated that the term implies "hair weight for equal length," and that its numerical expression as used in this book is in milligrammes per centimetre. The reciprocal of this expression is equivalent to a statement of the "count" of the hair.

No method for measuring the frequency distribution of hair weight is yet available, though the knowledge of this frequency would be of industrial value in assessing the liability of a cotton to the formation of "nep" (p. 114). The technique here described only provides us with the average value of this characteristic.

This technique has been simplified and speeded up since I

[1] *Jour. Text. Inst.*, 1924.

first employed it. The counting of the hairs is done with the microscope, thus avoiding eye-strain. The weighing is done on an Oertling assay balance, which reads to hundredths of a milligramme, and the minor calibration errors of a spring microbalance are thus avoided, the results being obtained in absolute measure, subject to moisture-content correction if necessary. The manipulation (p. 354) is simplified so that the whole test can be completed and computed in a quarter of an hour.

Repeated tests made in the way described will give results which rarely differ by more than 2%. This high degree of accuracy is, of course, due to the large number of hairs which can be thus averaged, while the experimental errors are such as may give plus or minus errors in equal proportion.

Hair Break.—The determination of the breaking load of the hair is quite useless by itself, but when made in conjunction with the previous test the results are very important. The hair weight having already determined the number of hairs which will be found in the average cross-section of any given count of yarn, we can then utilise the hair-break figure to compute the maximum possible strength of the yarn, or, as I have termed it, the Intrinsic Strength (Chap. XII).

The technique of this test has to take into account the fact that we here reach the maximum variability of the hair-properties, and also the danger of unconscious selection of individual hairs in preparing for the test. The latter error is liable to be serious, because stiff hairs are more easily picked up than fragile ones. In our early work we employed a small Wimshurst machine to make all the hairs stand out equally straight and conspicuous, but the preparation of drafted sliver for the Sorter provides a more convenient method; a tuft of hairs can be withdrawn with the fingers, or by the Sorter itself, and every one of these must be tested. They will make up a true sample if the sliver has been properly prepared, because the only basis on which the fingers or the instrument can possibly select them is by the position of their front ends in the tip of the sliver, and this—on the average at least—is independent of any other property of the hair.

A semi-automatic instrument for making this test was described in *Raw Cotton*. It was much modified into a magazine type of instrument, then modified still further until it became a purely mechanical arrangement which has now been in routine

use for seven years. This Magazine Hair-break Tester has been exhibited and supplied to other laboratories. The test is standardised upon a length of 1 cm. of hair, the rate of loading is about one gram in two seconds comparable with the usual rate in yarn tests, and exactly the same treatment is given to every hair.

The magazines are interchangeable, and hold fifty hairs apiece. A skilled operator can load one magazine in half an hour. A quarter of an hour suffices to read and plot the record from the previous magazine, and the instrument takes three-quarters of an hour to work through all the hairs on the magazine put into it. The testing operation can thus go on continuously, one operator and one machine keeping step.

In this test, more than in any other, we become unpleasantly conscious of the need for a micro-balance, or some other weighing method, which would be sufficiently sensitive and robust to weigh single hairs, or would sort them out in order of their weight-distribution around the known average value. Results of moderate accuracy are given by the testing of two magazines containing a hundred hairs in all, but this number is not sufficient for all purposes; it may therefore be necessary sometimes for the operator and the machine both to be working for a whole day on a single sample, although an hour and a half is usually adequate. On the other hand, if the dimensions of each hair tested could have been ascertained previously, instead of having to be averaged out by random sampling, the correlation between break and hair weight would enable the observed break to be corrected to the known true average hair weight, and the error of the test would be reduced to negligible proportions. Something can be done in this direction by microscopic examination of the hairs in position in the magazine, but the weaknesses of the microscopic methods, already described, prevent this from being of much use.

Some good examples of the existence of a sampling error, even with all the precautions I have described, will be found in the data for the Thirteen Samples.

Average values for the strength of large numbers of hairs may be obtained, without the tedium of individual hair-testing, by using an impact method. The meaning of impact tests upon cotton is obscure, however, as we shall observe in more detail when dealing with yarn (p. 228), and until it is better understood

(p. 258) we cannot safely allow the convenience and speed of the impact method to weigh with us.

Convolutions.—To the best of my present knowledge the variations in average number of convolutions are not important in the determination of yarn properties, and their measurement is not essential to the work of the testing laboratory. It would nevertheless seem probable that the future will show some such connection indirectly by way of the slipperiness of the hair, and it is therefore desirable to include this measurement in this chapter.

The measurements are easily made on the same slides as those prepared for ribbon-width, using a low-power microscope objective. The number of convolutions is counted along a fixed length of at least a hundred successive hairs, preferably in the middle region of each hair. This fixed length is conveniently defined by a rectangular opening in a diaphragm in the eye-piece of the microscope.

The average form of the convolutions can be expressed numerically by conjoint consideration of the convolution length, the ribbon width and the wall thickness.

Slipperiness.—No really satisfactory method for obtaining a figure by which to express the co-efficient of friction of the average hair upon its neighbours is yet available. It was only in the last year of the researches recorded in this book that I was able to isolate this factor of slipperiness from the complex of the yarn-cotton relationships, so that the provision of a routine test has not yet received the attention it deserves, though my existing method is ten years old.

It is obviously no easy matter to standardise the conditions of frictional contact with cotton, even in the simplest case of a single hair rubbing on a cleaned glass surface. The area of contact enlarges, and the hair surface loses wax to the glass, which is thus no longer clean, while the hair surface itself is thus changed as the test progresses. In some ways the testing of friction between hair and hair is less difficult, if only a statistical average is required, and the conditions are those under which the cotton is used in the mill, but the definition of the pressure applied seems almost insuperable. I had hopes that Adderley's method [1] would have been susceptible of development, but critical study of it has so far been disappointing in this respect.

[1] Adderley, A., "Clinging Power of Cotton Hairs," *J. Text. Inst.*, 1922.

The only dependable test as yet for these properties is the empirical method of testing the Untwisted Roving. The cotton is taken through the mill—itself undesirable, because it may make some change in the surface of the hair—up to the fly-frames. Roving from these is then mounted on an O'Neill tester, in which tension is steadily applied by removing the supporting water from a floating weight. The upper end of the roving is attached to a sensitive indicating quadrant balance, like a letter-weigher, and the maximum load attained is noted, all the twist having first been removed from the roving by rotating the lower float, after the roving has been set up without tension on it. This slipping-load is then divided by the average number of hairs in cross-section of the roving, as known by the hair-weight figure, and the average slipping load per hair is recorded.

The rovings having all been through the same machines, twisted up to the same lateral compression, and released from that compression by untwisting under identical conditions, this method gives surprisingly consistent results (p. 248). The average of twenty separate pieces of roving, each 10 cm. long, gives a dependable value, which we shall have occasion to welcome in later pages.

I would like to emphasise the empiricism of this test, as also its lack of convenience in that it cannot be made out of reach of a spinning mill. There is a possibility that the technique of experiments with the Porometer upon the cross-sectional area of the hair may yet cast up a suitable test for slipperiness as a side issue, or that impact methods will repay study (Chap. XV).

Minor Tests.—It is advisable to include some reference to three other possible testing methods of minor importance, which might become useful when suitable technique has been discovered, or when their significance is better understood.

Slip spirals.—Measurements of these may usefully be made with the same microscope equipment as that used for wall thickness (p. 349). In such balanced illumination the slip spirals show up conspicuously, presumably on account of some change in the boundary cellulose, such as Prof. W. Robinson [1] has described for slip surfaces in wood. Their numbers may be counted between parallel lines in the eye-piece, as for convolutions, but with a higher-power objective. In view of their artifact origin intro-

[1] *Loc. cit.*

ducing yet another factor into their determination, it is advisable to increase the number of hairs well above the minimum of one hundred.

Specific gravity.—The specific gravity of the wall is quite distinct from that of the cellulose, the former being a spongy structure. Measurements of the cross-sectional area are not sufficiently accurate under the limitations of the testing laboratory to make routine statements of apparent density worth while. At one stage of my work it seemed fairly clear that real differences existed as between different cottons in this matter of density, but the question is still undecided, for our latest critical results pointed rather to similarity than difference around values about half-way between the 1·55 for cellulose itself and 1·00 for water.

Elasticity.—Observations on elasticity, permanent set and total extension have not been developed to testing-room routine, and the inevitable tedium of these observations for a single hair, even with suitable apparatus, makes the prospect of repeating them not less than a hundred times for each sample quite impracticable. Their future development depends on the provision of single-hair weighing devices, the need whereof has already been mentioned when describing the hair-break limitations, or on fuller knowledge of cotton generally.

Flexibility.—Practicable and accurate experimental methods were made available for me [1] in the measurement of this characteristic, but again it seems unlikely that routine testing will be needed, because so much of the absolute value per hair is covered by the simple dimensional statements of fineness.

In the present chapter I have almost disregarded the influence of moisture content on the hair properties as they are measured in the testing laboratory, and this for two reasons. In the first place they have already been the subject of discussion for their own sake in a previous chapter. In the second place it has been my experience that the provision of standardised conditions for the temperature and humidity of a testing laboratory has a tendency to cause all persons concerned to regard the test results much too seriously. The errors of testing, whether of yarn or of cotton, are so serious on account of the variability of the material,

[1] By F. P. Slater.

of its pre-history and the like, that the additional errors due to humidity and temperature variations inside the average laboratory are merely additional. As the technique of testing improves, so the standardisation of these variables will become more necessary, but the number of occasions upon which it has thus far been found worth while to switch on the automatic temperature and humidity controls provided for our testing laboratory were very few indeed.

This topic is cognate to the importance of statistical treatment throughout the handling of test results, in order to assess the significance of any data obtained. I assume the reader to be familiar with the elementary theory of statistics, or to have such a book as Yule's *Theory of Statistics*[1] available for reference. In addition to such treatment of the results by rigid and formal statistical methods, the student of textile data must very often be content to make the best of the imperfect data obtainable by guessing at the meaning of graphs.

[1] Griffin, London, 6th ed., 1925.

PART II

THE YARN

CHAPTER VI

THE STUDY OF COTTON IN YARN

It is twelve years ago since I tried to arrest the attention of any readers of *Raw Cotton* by casting my general conclusions concerning the interdependence of Grower and Spinner into the form of a most thorough platitude: "Cotton is grown to be spun. If it spins well it is good cotton; if ill, bad."[1] This particular essay in the inane would seem to have been successful, for I have often seen it quoted. Yet in order to find the foundations upon which to build up all industrial research upon cotton spinning, we must first treat this platitude very seriously. Separately for the Spinner and for the Grower, as well as for both conjointly, it is necessary to ascertain how and why the properties of the yarn are connected with those of the raw cotton.

The elucidation of this connection is a very definite aim, to which all kinds of observations and experiments can be directed. Many of these have been planned for other ends, but their eventual application to this central object has served to interconnect them, and thus to reveal information which might otherwise have escaped notice.

In the early stages of the Experimental Department's growth, my attempts at a preliminary survey of these relationships were of necessity restricted almost entirely to observational methods, in the absence of any experimental mill. Though this restriction was prejudicial in the long run, it was probably advantageous at the outset. Suitable material for very full observation happened to be at hand in the form of Thirteen Samples of American cotton (Chap. XIII); these had been very thoroughly spun repeatedly in two mills, because there had been serious discrepancies in their spinning-test behaviour; I found eventually that lack of care had allowed the bobbins to be mixed during one of the tests. When they came into my possession the sets of

[1] *Loc. cit.*, p. 146.

roving bobbins had been tested again and again, and had been shown to behave quite consistently in the production of 90s full twist yarn. These bobbins and their cops of yarn were filed, and this material was not only studied by itself, but was used for years as the standard of reference, or touchstone, against which all new ideas were checked. Properties of the yarn were measured upon it, as soon as each new method of measurement had been devised, properties of the cotton were similarly determined from the roving, and the search for cross-connections was sedulously pursued.

This material was a peculiarly fortunate choice, in several ways. The problem was reduced to its simplest terms with respect to the cotton itself, because this was in the form of jack-frame rovings, so that if the previous preparation machinery had exerted any influence on the cotton, its effects were included as properties of the cotton. Then came an advantage in the opposite direction, for the count of the yarn had been pushed up much finer than would be practicable for commercial working of these cottons; differences between them were thus emphasised, and the defects of the yarns were rather exaggerated. The greatest advantage of all was that the number of samples concerned was so large as to make fictitious agreements very unlikely; on several occasions I reached explanations which fitted reasonably well for twelve of the samples, but broke down at an unlucky thirteenth. Many premature generalisations were thus suppressed.

As it is possible that my repeated reference to the Thirteen Samples may give the reader a false impression concerning the scope of these studies, it is perhaps worth while to point out here that these short American cottons lay well off the ordinary track of fine spinning. Most of my everyday experience was concerned with fine cotton and fine yarn, so that the inferiority of these samples was another of their advantages. In one way or another this Part utilises information drawn from the whole gamut of cotton-spinning, my yarn material ranging from 10s to 250s, and my spun cottons from Rough Peruvian to Super-St. Vincent; from Brazil, through Tanganyika, to Queensland.

Although cognisant of the limitations attaching to the observational methods which I was initially compelled to employ on the Thirteen Samples, it was not until some years later that I realised I had taken hold of the stick at the wrong end. I had taken a

series of yarns and had tried to interpret them in terms of the cotton used; the assumption was that the yarn properties provided my known quantity, and the cotton my unknown quantity. The yarn seemed to be the natural starting-point, the one stable thing in a shifting maze. Yet the most elementary logic will show that the correct procedure is the exact reverse; the cotton explains itself in terms of the yarn which it will make. Eventually I found that my real unknown quantity was the yarn; routine mill knowledge of its properties was limited to a few conventional expressions, quite adequate for working practice, but useless for analytical purposes. Each sample of cotton should be tried out in various twists, drafted with various roller-settings, spun with various tensions, two-folded with various arrangements, woven and knitted into fabric, and so have its possibilities explored; such exploration would not be confined to the range of working practice, but should stretch out to the limits of absurdity in all directions. All the resulting yarn properties should then be explicable because a certain cotton had been used. Until the resources exist which will enable the industry to do such experiments adequately, we must be content to fall back on a mixture of experiment with observation.

The first discovery provided by the Thirteen Samples was this: that almost every measurement needed was unobtainable until a suitable method had been invented; the few exceptions were only partial, because the significance of the conventional measurements lacked interpretation. The most important instance of the latter was the lea test for "strength" of yarn; because I was unconsciously working from the wrong end of the subject, I naturally took the lea test results as my starting-point; the samples were arranged in the order of their practical significance as it is defined by this test, and I proceeded to search for other properties which, singly or in conjunction, would give the same order of merit. Some two years elapsed before it became clear that the lea test itself required analysis (p. 233), in order to make the basis of interpretation secure. The rest of the usual testing-room equipment was quite inadequate to yield the information about yarn which I required, and a succession of new pieces of apparatus had to be designed, built, and studied, in order to gather such information.

In addition to the hair-testing methods already described in

Chap. V, this new apparatus (p. 348 *et seq.*) included an automatic single-thread tester for examining sequential variation along the yarn, the Yarn Dynamometer for studying the break and extension of any length of yarn, and an entirely new Impact tester; a semi-automatic Twist-yield tester for examining the relation between twist and the breaking-load, and Torsionmeter for the twisting couple; the Elastometer for discriminating between true elasticity and permanent set, the Yarn Clinometer for measuring twist in singles, and various devices for study of the variations in yarn-diameter which culminated in an harmonic analyser and Periodoscope. All these appliances and many other devices of technique and computation were made necessary by questions put forward through the Thirteen Samples alone. Each of them led to some discovery or simplification by substituting exact information in the place of vague impressions. Each additional piece of information made the situation seem more bewildering.

I have already discussed the enormous variability of the hair, which handicaps our study of the cotton itself. The study of yarn is equally handicapped; first, by the variability of the yarn along its length; secondly, by the instability of its structure. A strand of yarn which has been stretched, wetted, shaken, or dried has no longer its original structure. Hence it follows that the proverb about the strength of a chain residing in its weakest link does not apply to singles yarn; especially is it inapplicable to bad yarns like those of the Thirteen Samples. The weak links become stronger under tension, if a little time is available for readjustment of the twist, and the places which were strong become weaker. The weak-link theory breaks down in some cases so completely that I have known the length tested in single thread to be increased from 5 cm. to 6,400 cm. without any definite alteration in the breaking load. So the question arises, as it did with the raw cotton—what is the "strength" of yarn?

Although any possible effects of the preparation machinery upon the cotton were eliminated from the material of the Thirteen Samples, it was possible that the mule itself might not be giving a true translation of properties from the roving-bobbins to the cops. The possible error thus introduced had to be taken as read, and it introduced an additional uncertainty into the situation. It was not until eight years later that the existence of

such an error was proved, and its nature identified, in the form of "nip-ratching" (p. 94). Fortunately it is itself dependent on the properties of the cotton, provided that all the samples under comparison are being spun side by side on the same mule, as the Thirteen Samples had been.

The reader will notice that this discussion returns continually to the subject of Strength, to the neglect of other properties of the yarn. That it should do so is, to my mind, inevitable. The strength of cotton is its one permanent asset, being exhibited in conjunction with the flexibility which is common to all textiles. It is not many years ago since cotton was regarded rather contemptuously as a mere cheap covering material, but it is no longer cheap, and it will become still dearer while the cotton-growing areas are developing their civilisation through this same practice of cotton-growing (Chap. XVII). It is not a beautiful material in the sense that silk and artificial silks are beautiful. It is not valuable for resistance to moisture, whether in clothing or in electrical machinery, as is cellulose acetate. Its one outstanding asset is in its power to endure reciprocating stresses, like those in the wall of a pneumatic tyre; until another fibre is synthesised or discovered which can equal cotton in this respect, it is secure for such employment. At the best its strength is not far behind that of mild steel (p. 52), in combination with a flexibility which no other engineering material possesses.

The fundamental importance of strength in yarn is not always obvious in textile processes. Strength seems to be quite immaterial in the spinning of soft doubling wefts; similarly the doubler will object that he does not double up to the maximum possible strength, but only to such strength and softness of the two-fold as will work well in the knitting frame; so also in the production of weft for weaving, the strength is quite incidental and is indeed kept down to a minimum, because each additional revolution of the spindle adds to the cost of production. But a cop of single yarn is no more a finished product than is a can of card silver; the cop is nothing more than a convenient form in which the cotton is carried along to the next process; similarly for most two-folds. The only thing that matters is the strength of the flexible finished product, where the individual hairs are not merely bound together by the twist of the yarn, but by the intercrossing or interlacing of various strands of yarn or hairs,

or by the same strand or hair turning back upon itself. Whatever may have been the yarn strengths in the intermediate stages, the strength and flexibility of the sewing cotton, hosiery, or cloth is in the last resort dependent directly upon the strength, and the slipperiness, and the flexibility of the cotton hair itself.

An interesting inference seems to follow naturally from this line of reasoning. Whatever may be the demands of the existing equipment of existing mills, the "strong" cottons will eventually be the only ones worth growing. That the market could easily be glutted with such cottons to-day is not relevant to the future, when they will be the only survivors of cotton against competing materials (Chap. XVII). An adumbration of this specialisation may be quoted from the time when Sakellarides cotton was first introduced into Egypt; the Government was repeatedly advised that the mills could only use a small fraction of the total possible crop of Sakel, so that over-production would be ruinous to the country; but the fellah planted Sakel everywhere, and the mills absorbed the whole.

The examination of yarn strength will be facilitated if the reader memorises the two Paradoxes (p. xvii). The First of these was: "The weaker the hair, the stronger the yarn." This paradox deals directly with the importance of fineness, and indirectly with the importance of uniform hair distribution in the act of drafting. It is unfortunately true that even a moderate approximation to such uniformity is never obtained in the spinning machine, but meanwhile this leads up to my Second Paradox.

The demands made upon the cotton while it is progressing through the preparation machinery are quite different from those made when it has eventually been twisted up into yarn. This Second Paradox may be phrased thus: "Up to the front mule roller, cotton must be slippery; afterwards it must be sticky." All that matters until drafting is completed by the passage through this last roller is that the hairs shall be free to slide upon one another with the minimum of mutual friction, each hair thus being free to take up the place assigned to it by the machinery, without disturbing its neighbours; the cotton should behave, in fact, like a viscous fluid composed of enormously elongated particles, and the lower its viscosity the better. The factors which influence this hair-to-hair friction include the

straightness of the hair, which is mainly controlled by the preparation machinery; they also include properties of the hair surface, which are specific to the cotton itself, with modifications due to such causes as weathering in the field. But once the drafting is finished, then the twisted hairs in the yarn should lock up on one another with the maximum friction, so that the minimum of twist may be used; an extreme example of this was provided by a colleague, who pushed up the strength of a soft weft yarn to the maximum which the same cotton could give in full twist, by so treating it that the coefficient of friction between hair and hair was sufficiently increased, without any concomitant weakening of the hair.[1] We must remember the flexibility required in the finished product, and not push this Second Paradox too far, for another extreme example may be taken from the doping of bleached yarn with cellulose solutions; the hairs are so intimately soldered together that we reduce the strength of the singles yarn, because the readjustment and redistribution of stress between hair and hair are prevented; such "yarn" breaks like a piece of uneven wire, at the thinnest place.

My Second Paradox thus deals chiefly with the Drafting process, which is of fundamental importance to all textile industries. The art of yarn-making from such discontinuous fibres as cotton, wool, or flax depends on a drafting process whenever strength is required; when mere covering power is the objective, "condenser spinning" will suffice, but drafting is a necessary part of even such hand-spinning as the making of haybands in the farm-yard. Before glancing at its primary significance in my own problem, it is advisable to note a certain confusion of ideas about it.

This confusion can be illustrated by quotation from any text-book of cotton spinning. Taking down the first such text-book which comes to hand on my bookshelves I find the following: "The gradual parallelisation of the fibres as they are passed through the rollers induces a more even thickness of sliver, especially when the drawn fibres are themselves combined and passed through the machine once or twice. The mere attenuation of the sliver, if it does nothing else, reduces

[1] See also Trotman, S. R., and Thorpe, E. L., *Principles of Bleaching and Finishing Cotton*, London, 1918.

the existing irregularity, and when repeated two or three times practically corrects it." The existence of some appreciation of ambiguity in this writer's mind can be read between the lines in his use of the words "especially when"; but he gives us the definite statement which is common throughout the cotton industry, that attenuation by drafting makes the sliver less irregular, whereas the truth is exactly the reverse. This confusion arises from failure sharply to separate the essential defect of drafting—that it makes the sliver more irregular than before—from the subsidiary statistical consequences of the fact that opportunity is taken to superpose two or more slivers, so that the variations in thickness of each are smoothed out in the mean of them all. This latter effect is, of course, discussed at length by the text-books, but the essential part of drafting, which is quite independent of such folding, is not disentangled from it. Turning to a later page of the same text-book already quoted, where the slubbing-frame is described, one might hope to find this disentanglement effected, since the slubber is fed with only a single end of roving, and the pernicious effect of drafting is not obscured; a trace of ambiguity is again visible, but no clear recognition, for the only reference to drafting in eighty pages of text comes at the end of a discussion of traverse motions. The traverse motion is shown to involve a risk of disturbing the effective roller weighting, and therefore "unless the draft exercised upon the sliver or slubbing is constant, the tendency towards thick and thin places in the resulting roving is much increased, and anything which detracts from the uniformity of the action of the rollers is necessarily hurtful." We again meet the indirect assumption that the action of the rollers on the cotton is normally uniform, while the fact that a traverse motion makes thick and thin places which are many feet long, not short ones, is lost sight of in faithful adherence to the belief that drafting improves the sliver.

I have taken these quotations entirely at random. They could be paralleled from any other text-book, but my object in giving them here is to make it clear that a dogma has been widely accepted which, as I shall show later, is devoid of any justification. The correction of this erroneous view is essential to any understanding of yarn properties. Each drafting operation converts the individual sliver into a more or less beaded

arrangement of thick and thin places, this alternation being the inevitable consequence of the present drafting process, though in the early days of cotton-spinning it was disguised and overlaid by less subtle irregularities such as can be provoked by imperfections in the roller-coverings, gear-wheels, and other parts of the machinery. The relative amplitude of these beadings becomes greater as the roving becomes thinner; in the draw-frames and slubber they are barely noticeable without quantitative measurement when the machinery is skilfully handled, but they are always present. When six or more slivers are superposed in the draw-frames, the smoothing effect of this (p. 126) is more than enough to compensate for the "irregularisation" which is going on simultaneously. Consequently, the finishing draw-frame sliver after three such foldings is the most regular thing in the mill; this is common knowledge, but its implication has been disregarded. Subsequently, while passing through the fly-frames and the spinning machine, the diminished foldings are not sufficient to compensate for the increasing relative amplitude of the drafting irregularity, and matters get steadily worse.

The finished singles yarn is thus a succession of thick and thin places, built up in a lamination of complex patterns by the drafting process; the degree to which it deviates from the ideal cylindrical form being dependent on the fineness of the yarn, and the kind of cotton employed, as well as upon the skill of the machine-setter and operator. We shall see that the self-adjusting properties of yarn (p. 172) help to minimise the weakening effect of these thin places; indeed, if yarn behaved like wire, devoid of self-adjustment, cotton-spinning would be almost impossible in the finer counts. Still, the yarn does consist of an alternation of weak and strong links, and when other things are equal its strength depends upon the frequency distribution of these local strengths; hence the strength of a badly drafted yarn falls off very rapidly down a "Strength-gradient" when the length under test is increased, and we shall see that this relation between length and strength is of fundamental importance in arriving at our final analysis of "strength" in yarn (Chap. XII).

The fact that this gradient, or "length-strength relationship," should serve as a measurement for the efficiency of drafting was obvious at a very early stage of my researches, but it was many years before the obvious became practicable, simply on account

of the many experimental obstacles which are planted in the observer's path by the instability of yarn structure. When these are overcome we find that this relationship is connected with the slipperiness of the cotton hair, and with its fineness, while the final breaking load of the weakest link in the stretched yarn is determined by the intrinsic strength of the hair (p. 71). The story is, after all, quite simple.

We can now revert to my platitude about the bad spinning of bad cotton (p. 75). Failing this direct understanding concerning the nature of yarn properties, we have been until now dependent upon the test of actual spinning; [1] moreover, we shall continue to be so dependent for a little while to come, until the partially qualitative analysis presented in this book has been placed on a quantitative footing in the study of hair slipperiness. But additional spinning tests add nothing in themselves to our general knowledge; a cotton-breeder may produce a cotton which is demonstrated by the test of spinning to be a good one, but the spinner has been unable to explain to the breeder how and why it was good, point by point. I am aware that attempts at such explanation have been made, but—living in both camps—I have also heard the plant-breeders' subsequent comments upon them. Could such explanations be given, the breeder could begin again and improve on his first result (Chap. XV). The information given by the spinning test may be final and complete for any particular sample, but it is sterile and useless for the advancement of knowledge, unless the properties of raw material and finished product are simultaneously analysed and interconnected. If the resources of agricultural science and craftsmanship are to be made available for the spinner, he must prescribe the details of his needs.

Thus my ultimate objective in starting work on the Thirteen Samples was, though it seemed hopeless, the construction of a Prediction Formula, by which the cotton-grower might be able to anticipate the result of a spinning test. I am certain now that such prediction is not only possible but will soon be practicable. After some minor accessory factors have been eliminated from the actual spinning we are left with two groups of yarn properties, both directly traceable to properties of the hair. One group contains the tensile strength of an ideal yarn, which

[1] W. L. B., *Handbook of Spinning Tests*, Macmillan, London, 1920.

is uniform in cross-section; the other is concerned with the deviation of the actual yarn from this ideal uniformity. Prediction of the first is rather more complex than prediction of the strength of a wire rope. Prediction of the second merely requires intelligent anticipation of the drafting performance, and although neither the spinner nor the research worker knows enough as yet to make this anticipation quantitatively, yet the fragments of the puzzle are piecing themselves together (Chap. XIII).

It is worth noticing that as our comprehension of the spinning test thus develops, so we shall be able to simplify its technique. I have already been able to obtain useful information from half-gram samples. Also, the main administrative difficulty when conducting spinning-tests is to produce a sufficient variety of yarns; one can get more information from bad yarns than from good ones; further knowledge might conceivably enable us to go to the extreme of spinning by hand, with no more expense on equipment or power than the housewife expended on her primitive spindle, and yet we might deduce from the yarn thus produced what result would be given by the mass-production technique of our modern spinning mill.

These general remarks should enable the reader to pick up the main thread of the argument, and the following chapters will expand particular aspects of the present one. Some such elementary knowledge of mill processes is desirable as I have formerly presented in my handbook of *Spinning Tests for Cotton-Growers*; failing this, the following chapter on "The Machines" should be sufficient guidance. The main object of that chapter is a technical one, to show how and where the mechanical processes interfere with straightforward interpretation of the yarn properties; its main conclusion is that such interference is relatively insignificant, on account of the skill with which the machinery is handled, except in the fundamentally important process of drafting.

The ground being thus cleared, I proceed to outline the salient peculiarities of yarn structure, and deal in more detail with the nature of yarn instability and strength. The influence of drafting on the weak-link distribution, and that of twist with respect to the grip of hairs on one another, require separate chapters (Chaps. VIII and X). The accident of my invention of the Sorter has made it possible to learn so much by controlling the hair length

in various yarns that the preliminary experiments made in this way deserve an intervening chapter to themselves. After noting some characteristic features of the yarn as a structure in Chap. XI, we pass to the last two chapters of this Part, where I have first discussed the nature of "strength" in general terms, and then have demonstrated the application of all these previous pages to the touchstone of a fairly complete statistical analysis for my Thirteen Samples. The solution of their problems, as given in the thirteenth chapter, makes no pretence at finality in a quantitative sense, but it is at least consistent with all the measured facts available from all the cottons and all the yarns which I have so far met, and it bears internal evidence of reasonableness.

CHAPTER VII

THE MACHINES

Disproportion; invention; output; function—The Mule; craftsmanship; B.T.P. diagrams; nip-ratching; chase-break; tension; winding; assets of the mule—The Ring; break-places; tension—The Rule; a ring-mule hybrid; arrangement; advantages—Combers; ineffective as length sorters; slipperiness factor—Cards; fineness fractionation—Changes in the cotton; slip spiral; neps; break; length—Machines of the future.

THE mechanical equipment of a cotton-spinning mill has a strong fascination, even for casual visitors who are in no way mechanically minded. Much of this fascination is due to that enormous discrepancy between the machine and its task, between the mass of metal and the film of cotton, which makes cotton-spinning look like using a steam-hammer to kill a mosquito. If we consider the power applied, and the power expended on the cotton itself in the acts of withdrawing single hairs, straightening them, laying them in order, and twisting them together, the mechanical efficiency of a cotton mill is negligible. The reader could make a rough estimate of it from some of the data in these pages. The machines, then, are clumsy in spite of their ingenuity.

From this clumsiness there follows the big capital cost of the machinery, and its floor space, from which in its turn follows the need for cheap construction of the machines themselves. This construction is particularly interesting when put in comparison with the type of construction used in modern agricultural machinery, where the machine is strictly adopted to its work by methods which were very unconventional when such an implement as the self-binding harvester was first introduced. In cotton-spinning, on the other hand, the work is in some degree fitted to the machine, because the modern machinery is still the machinery which it was possible to make a hundred years ago. Thus, until quite recently the carriage of the mule showed clear traces of its growth-history, the short carriage of the early mule having been made longer by adding bits to it when longer mules became practicable; it is now beginning to undergo constructional

changes, for it has become a long slender girder, and the need for its re-design as a girder has become apparent. Similarly, the joining together of rollers, end to end, in the fitting-up of a roller beam has only recently deviated from the village blacksmith's method of 1800; a tapered square on one end fits into a square hole in the next end, this being almost the worst possible method to use when exact alignment of the two is required; at modern speeds of working it has been found that this survival of the smithy is inferior to properly centred and drilled holes, with plug and key-way, which modern resources have had available for years. Thus the mechanism of the modern Spindle is in some ways more backward than that of the modern Hoe. Coming into cotton-spinning with an amateur interest in engineering, and some personal knowledge of instrument design and construction, I have found this aspect of the machines very suggestive; they present something akin to a geological succession of engineering resources, often in a better state of preservation than is warranted. It seems inevitable that the process will be rendered less clumsy in course of time, and that the amount of semi-skilled attention will be much reduced.

The history of invention among the machines is a remarkable contrast to the history of their utilisation. Starting with Wyatt's wonderful drafting-roller at the time of "the Forty-five," the inventions are almost finished with Roberts' self-acting modification of Crompton's mule in 1825. The century which separates us from Roberts has seen only one novelty, Heilmann's comber of the 'fifties. Many details have been altered; the comber was brilliantly re-designed by Nasmith; but the machines fundamentally are the same as they were a hundred years ago.[1] This lack of radical change has enabled an immense amount of attention to be given to details, which is often rather disproportionate to the little we know about the reaction of the actual cotton to the machines. I think it is very probable that much which was conscious knowledge in 1800, based upon trial and error, or even upon direct experiments made by the founders of the industry, has been forgotten during the passing years, or has been conventionalised, and so distorted; while I was visiting the Bolton

[1] *Vide* Dobson, Sir B., in *Some Difficulties in Cotton Spinning* (Manchester, 1907), describing Crompton's own mule as "containing . . . an equivalent of every motion of . . . to-day."

Museum with a friend who had improved a detail in the lay-out of creels, we found that his improvement was the original form employed in one of the museum specimens; both he and the founders of the industry were right, but custom had deviated into the wrong form at some time during the intervening century.

If we now turn to the history of their utilisation, we find the reverse of this stagnation. The output from a spindle has been roughly trebled since Heilmann's time, as the accumulated result of improvements in details which are individually trifling. Machines can be run faster, stoppages are shorter, and the industry is now largely governed by considerations of output. Unfortunately there is a natural antithesis between rate of output and quality of output; even within my twenty-one years' experience of cotton I have observed certain products replaced by inferior ones, and have learned afterwards that the drop in quality was due to a change in manufacturing methods for the sake of bigger output; there is no doubt that in some cases the demand for cheapness has made it difficult to obtain goods of the old quality. If I am right in concluding (Chap. XVII) that the artificial silks will presently compete seriously with cotton for general-purpose uses, this partial disregard of quality in cotton can only be a temporary phase, destined to disappear when cotton has to make good its position on the merit of its strength.

Meanwhile the cotton industry as a whole has become highly specialised and standardised in its processes, to a far greater degree than is the case in the kindred industries of woollen and worsted, where the refinements of output technique have yet to be standardised. Cotton is more highly specialised than wool, more severely divided into water-tight compartments; by so much the more is it difficult in consequence to alter any part of its procedure; knowledge must be very sure and fundamental before such alteration can be attempted with any hope of success, on account of the enormous capital invested in the long-lived machines.

I do not propose to deal descriptively with the existing machines, but shall take them for granted, except where some feature of interest seems worth mention. In the *Handbook of Spinning Tests* [1], which I prepared at the instance of the Empire

[1] *Loc. cit.*

Cotton-growing Committee, I attempted to eliminate the usual mechanical details from the story of the passage of the cotton through the mill, concentrating attention instead upon the functions of the machines. Since that was written I have had frequent opportunities for demonstrating mill operations to visitors, and have found this concentration upon function very helpful. It has the advantage that one is under no obligation to explain how the function is achieved, but simply takes its performance for granted; our knowledge of some of them, such as the drafting process, is not yet in a fit condition for explanation.

The functions of the machines, in terms of the single hair, may be separated as follows:

Mixing.
Cleaning.
Disentanglement.
Parallelisation (also implying straightening).
Fractionation.
Equalisation of number.
Reduction of number.
Twisting.

The allocation of these functions to the different machines is rather confused. Mixing begins at the bale-breaker and reaches its climax in the finishing draw-frame, but is continued until the actual spinning. Cleaning is practically completed at the card, so far as extraneous matter is concerned. Disentanglement proceeds to a climax in the card or comber, but is continued by every subsequent drafting. Parallelisation is improved after the comber by each drafting process until the finishing draw-frame, and the condition then reached is preserved as much as possible by keeping the roving slightly twisted in subsequent operations. Fractionation is as yet the undeveloped function (p. 165); a very little fractionation takes place in the card, rather more in the comber; but we shall see later (p. 110) that the comber is a poor "sorter," its value residing in disentanglement (especially from neps) and in parallelising. Equalisation of number of the hairs in cross-section is primarily the task of the folding which is practised in the draw-frames, though the average number is regulated further back, and it is common knowledge that matters go steadily worse after leaving the draw-frames

(p. 83). The reduction of number is incidentally effected by cards and combers, but is the prime concern of every set of Wyatt's drafting rollers. Twisting is a mere accessory after the fact of parallelisation in the fly-frames, but it is the principal task of the spinning machines themselves.

The drafting rollers (Chap. VIII) thus take a part in every function except fractionating and twisting, the cotton having been cleaned before it reaches them; there is slight evidence that they even do a little unintentional fractionation. It is all the more surprising that they have for so long been taken for granted, while technical attention has concentrated upon more impressive appliances of less real importance.

The degree of influence which each machine exerts upon the production of quality in yarn—a classification which is quite distinct from the feasibility of controlling that production by skilled attention—is greatest in the mule and least in the bale-breaker, on account of the importance of drafting (p. 128). For this reason it is desirable to work backwards through the machines in following up such topics of special interest as they have presented to me.

The Mule.—My experience of this extraordinary machine is a particularly striking example of the importance of Method to the research worker. For many years my colleagues and I felt helpless in face of the accumulated knowledge of four generations of spinners, who had worked on and round and under the mule, handing on their technique for its adjustment from father to son. It had been described and discussed in the text-books until one had an uneasy feeling that it was a mechanical monstrosity, entitled to reverence as one of the most ingenious of all machines, and yet impracticable by any ordinary standards of judgment. If we may imagine the record of invention rolled up, so that the men of to-day could be confronted with the modern mule as a new invention, I have not the least doubt that the inventor would be unable to sell a machine to any spinner; the size, cost, complication, slowness, and the peak-load power demand would separately and collectively outweigh any claim or any demonstration concerning the quality of its product. The mule seems to stand in the same relation to us of to-day as does a Dinosaur—except that we happen to be used to mules.

If regarded simply as a piece of physical apparatus for

producing yarn under controlled conditions of tension and twist, so that we may hunt down the relationship between cotton and yarn, it is seen to be nearly as defective as any apparatus could be. Successful conduct of tests upon it is only made possible by such meticulous attention to detail as is the accepted tradition for the mule, but would be prohibitive if expended on laboratory apparatus. The insertion of twist, which is its primary function, is dependent upon belt drive, so that even in a testing-mule an odd spindle band may slacken off without being noticed, thus producing a mile or two of yarn with sub-normal twist. The tension on the yarn while spinning is controlled by ropes, which have to be adjusted frequently, because their length varies with atmospheric conditions. The tension while winding the spun yarn is not only affected by the loading purposely given to it, but also by the friction of a fifty-foot bar of steel tube, which is supported in many bearings along a whipping carriage. Yet the mule is made to work, and the differences in its working from one time to another, under skilled control, are not sufficient to prevent it from continuing to work.

In face of the accumulated experience which has achieved this almost impossible feat for its daily bread, such an outsider as myself could feel very little hope of reducing these innumerable variants to an orderly scheme of knowledge, or of producing any observations which could be new to cotton-spinning. It is true that in my earliest observations on yarn I had looked for, and found, the fall of twist from spindle nose to roller which I term the "mule-draw effect" (p. 178), afterwards described independently and in detail by Dr. Oxley;[1] this was an indication that the wisdom of experience was not yet quite complete. But I was quite unprepared for the way in which my colleagues and I became able to speak authoritatively about features of the mule in the course of a single month, when the method for its study had been found and applied. This method consisted in plotting, from simple observations, a diagram which serves a similar purpose for the mule to that which the indicator diagram serves for an engine.

The object of this diagram is simply to place on record, in graphic form, the Time and Place at which any one "end" of yarn breaks down. By suitably presenting these facts in relation

[1] Oxley, A. E., "The Regularity of Single Yarn," *J. Text. Inst.*, 1922.

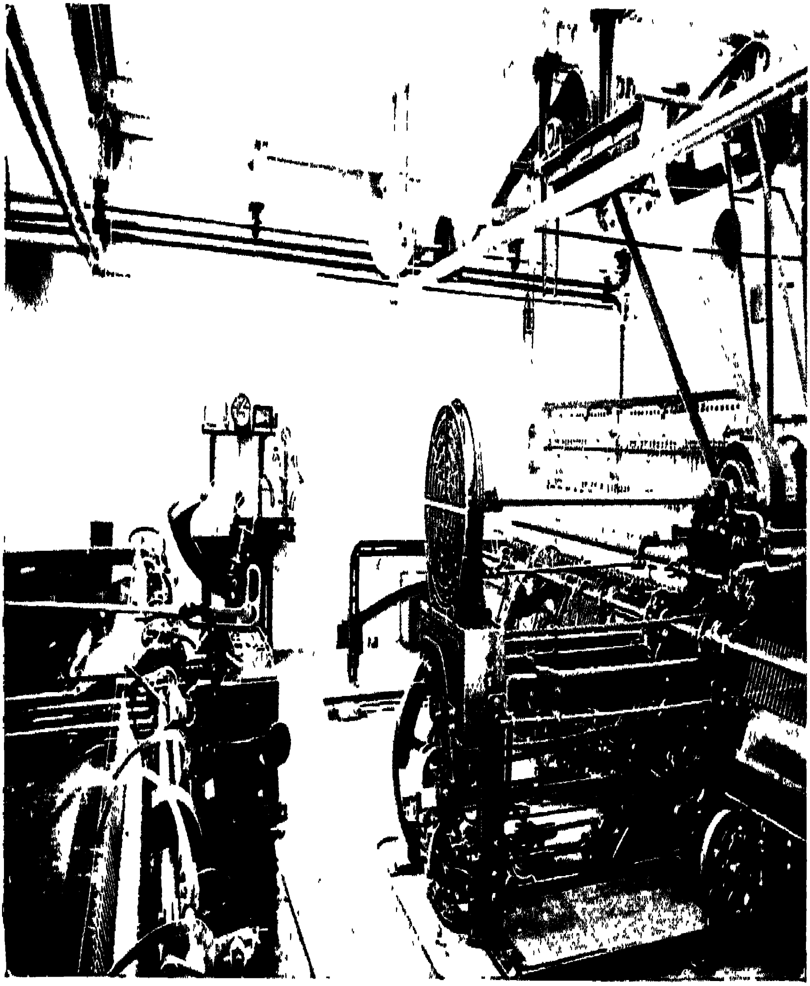

Fig. 23.—Short Testing Mules in the Spinning Section of the Fine Spinners' Experimental Department.

[To face p. 92.

to the mule's cycle of operation, inferences about the causes of the Breaks can be drawn with ease. The mule observed is not subjected to any experimental modification, and its treatment of the cotton is deduced from the behaviour of the cotton itself. It is described as the "Break-Time-Place diagram," or, for convenience, the "B.T.P. diagram." Though I started the observations of break-time myself in the course of a suggested investigation into the causation of certain breakages, it was at the instance of a colleague [1] that the place of break was also recorded, and the diagram made possible.

It will be more convenient to describe the method of making these diagrams at this stage, rather than in the Appendix, because no mechanical appliances are required. A group of spindles is selected for observation, and preliminary observations are made with the stop-watch in order to find out what length of yarn is being delivered in successive seconds during the outward run of the carriage; the duration of the four stages of the cycle are also timed, namely, run-out, spinning at the head, backing-off, and winding. The framework of the diagram is drawn from these preliminary data, showing the state of affairs at each second of the cycle, as if a succession of plan-views of the average spindle had been presented kinematographically.

The observer then starts the stop-watch at the beginning of each cycle, and stops it whenever an end is seen to break down in the selected group of spindles. The broken end is removed from the spindle tip and measured to find its length, and so the place of break; this measurement may be done directly or by inference from weighing. This length is plotted as a dot on the diagram, measuring from the part of the frame-line which shows the position of the spindle tip at the ascertained time. A glance at the diagram then shows whether any one break took place at the spindle tip, or at the roller nip, or whether it was at any intermediate position; the same glance also shows the exact stage in the cycle of operations at which it happened.

The framing of the diagram can be made more readable if we mark out and join together by "contour lines" those points along the yarn in successive seconds which are at all times equidistant from the spindle tip. Breaks which lie along any one such line are presumably due to the same initial weakening

[1] Mr. H. A. Hancock.

cause, because they are in the same part of the yarn, though the act of breakage has been delayed. On reading the diagram with the help of these contour lines it is productive of most useful information, over and above the mere record of the timing of all the operations which we took in order to draw our frame-lines.

In all kinds of spinnings and settings there is a strong tendency for breaks to be concentrated, as a cluster of dots, right up in the nip of the rollers at about the first third of the run-out; this is merely a confirmation of the well-known fact that the design of the mule causes "gain" to be at its maximum at this stage, with a corresponding maximum of yarn tension also (p. 100). The tendency of the dots to accumulate at the roller nip is an indication that the weak spot of the yarn is there, where the twist is being put into it.

If we now pick up the contour line which starts at this group of dots, and follow it onwards, we find a marked tendency in most settings of the mule for later dots to concentrate along this line. This is a graphic indication of the fact that the yarn has been stretched or "ratched" at the nip, almost to the breaking point but not quite, for a few hairs have been caught by the twist and have thus delayed the actual breakage for a few seconds. With ordinary spinnings and settings it is possible to trace this trail of postponed breakages right across the diagram to the winding stage.

While the excessive tension at the first third of the run-out is thus responsible for a very marked effect, as shown by the cluster and by this trail which keeps at a constant distance from the spindle tip, the Nip-ratching effect is not restricted to this momentary provocation. A second trail emanates from the same cluster, and this one keeps along the line of the roller nip, showing that the roller nip is still a weak spot, even when the tension is diminishing. Thus it is possible, in records taken from mules running under normal mill conditions, to trace eighty or even ninety per cent. of all the broken ends, directly or indirectly, to a cause which operated at the moment when the sliver was emerging from the nip of the rollers. This cause can only be the failure of the spinning yarn to gather up the emergent sliver hairs, or, conversely put, the failure of the hairs to put themselves in the path of the gathering action of the spinning yarn; for instance, it would be intensified by any "licking" round the rollers. I have employed the term Nip-ratching to describe the result of such

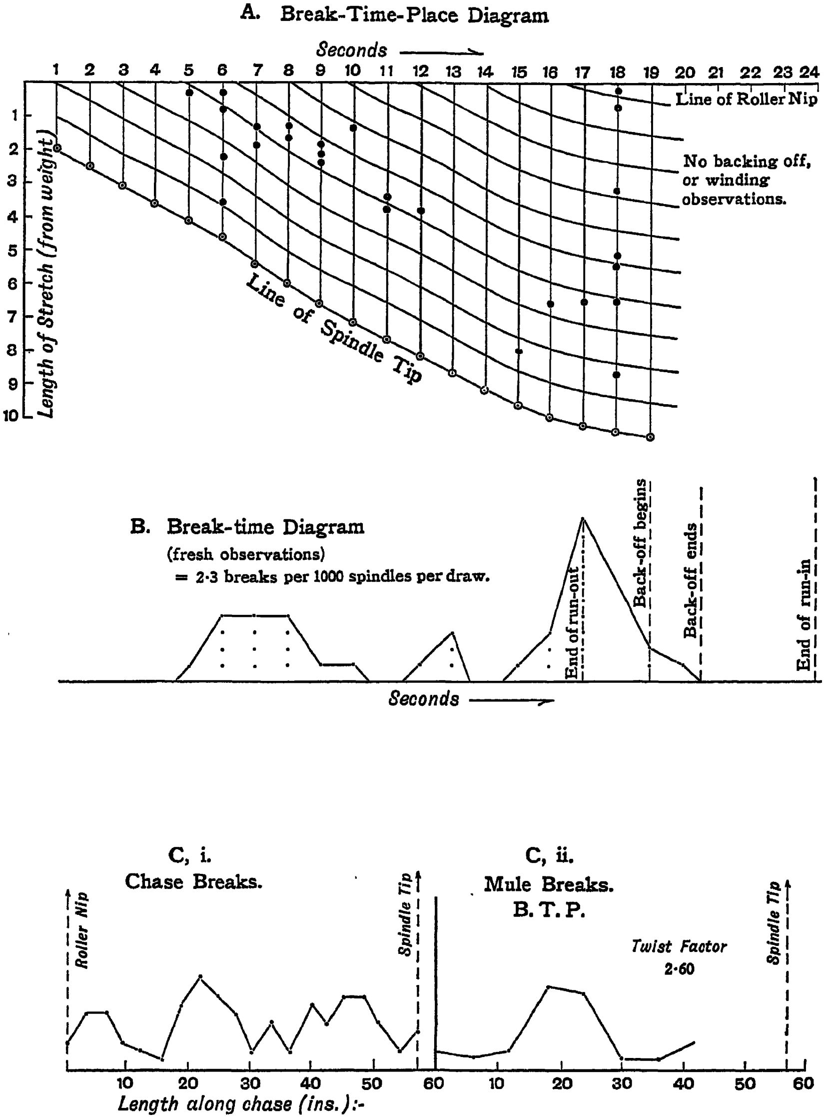

FIG. 24 (A).—Break-Time-Place diagram (B.T.P.) from a testing mule, under running conditions which caused high tension in the fifth to seventh seconds, and again while spinning at the head.

(B) Break-Time diagram of the same, observed separately.

(C, i) Break-Place diagram ascertained from A by summing along the contour-lines which show equal distances from the spindle tip.

(C, ii) Another Break-Place diagram made subsequently from the yarn thus produced, in the form of a Chase-Break curve.

failure, whether the resulting break be instantaneous or delayed. I shall presently (p. 98) show that it may even be delayed until the finished yarn is brought to the testing laboratory or into actual use.

I have not been able to find any previous knowledge of this phenomenon, but apart from its novelty and its outstanding interest as a fundamental defect of the mule—and also of the ring-frame—the B.T.P. diagram of any mule is bound to be informative. Thus we found on one occasion that there was a very unusual accumulation of breaks along the frame-line of the spindle tips during the run-out of a mule which had not previously shown any such tendency. It so happened that a colleague had been studying the critical speeds and vibration phenomena of mule spindles, and had determined these for the spindles of this particular mule. On comparing the data we found that in this particular setting we were working during the run-out at a spindle speed which was very near a critical speed, and although the specially picked spindles of this testing mule were not showing any marked vibration, they were evidently vibrating enough to cause an unusual abundance of spindle-tip breaks. These breaks disappeared from the B.T.P. diagram when the speed was altered.

Spindle-tip breaks are generally common while the mule is spinning at the head, presumably on account of the hardness of the yarn at that point on a double-speed mule. The way in which this twist is imparted will be noted in a later chapter (p. 177), and, taken in combination with the high tension reached while spinning at the head, it is rather remarkable that such tip-breaks are not more abundant. The causation and abundance of breaks which happen during backing-off and winding may be similarly investigated by mere inspection of the diagram.

I have had opportunities for testing the personal opinion of the skilled mule craftsman against the presentation of facts afforded by the B.T.P. diagram. Just as the craftsmanship of grading for length fails to stand the quantitative test of sorting, so I have been interested—and surprised also—to find that the craftsman often fails to observe even the bare facts revealed on the diagram, quite apart from any inferences based upon their simultaneous availability. The diagram requires no apparatus but a stop-watch, and a ruler, or weighing device, together with a moderate expenditure of time in severely concentrated attention.

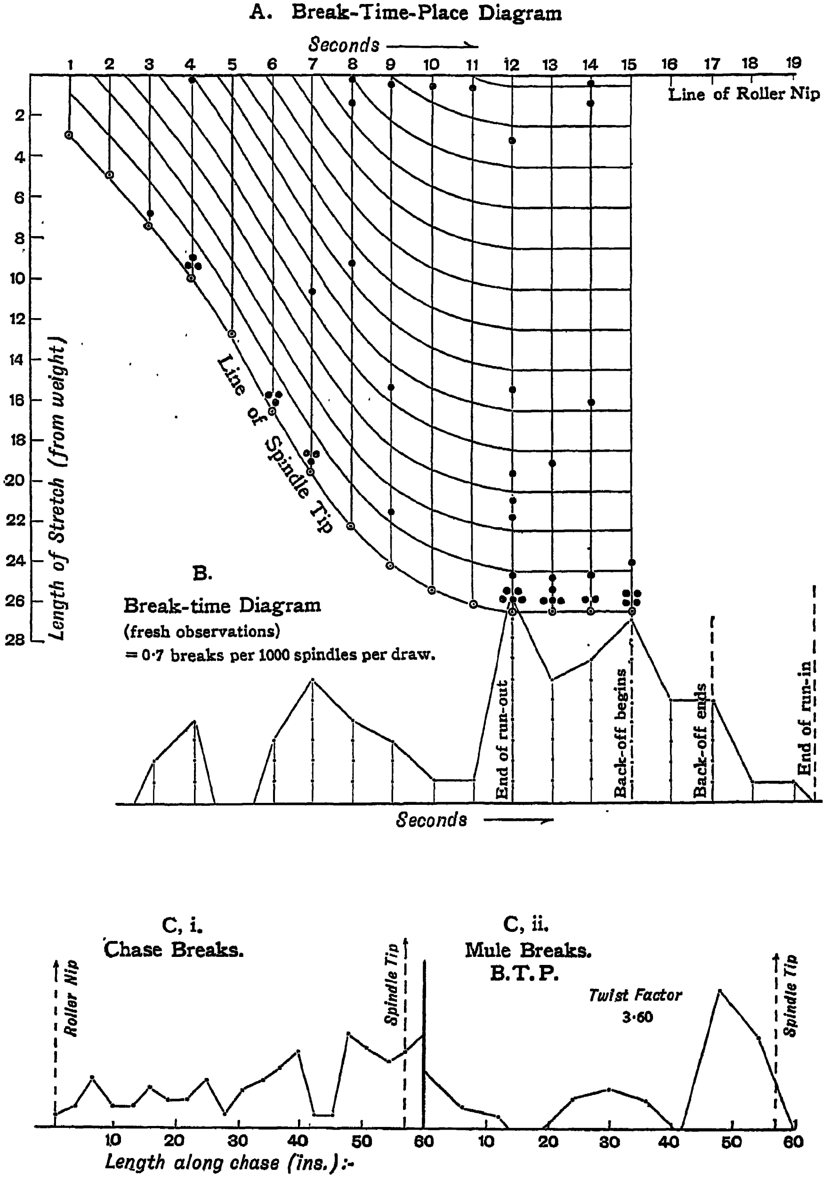

FIG. 25 (A).—Another Break-Time-Place diagram (B.T.P.) from the same mule under different settings, and with its spindles running at a speed very near the critical speed at which spindle-vibration was maximal.

(B), (C, i) and (C, ii), as in Fig. 24.

The art of reading it can be grasped in a few minutes, but if it is used for quantitative purposes, its reader should bear in mind that the time of exposure to the opportunity for breakage is a variable quantity. Yarn near the spindle tip is exposed all through the run-out, whereas the last portion which the rollers deliver is only exposed during the remaining operations of the cycle.

Following upon our preparation of these diagrams for a series of different mule-settings, I had the resulting yarns broken in such a way as to ascertain the place of breakage in each chase (or layer) of the cop. The chase corresponds to one draw or cycle of the mule's operation, and the position occupied by the spindle tip on the chases tested can be ascertained. A hundred chases or more can be examined very rapidly, simply breaking the chase by hand over a strip of one-way plush beside a graduated scale, and noting the place of break with respect to the spindle tip. On plotting the results in a "Chase-break Curve" they give a complex form which changes from one setting to another of the same mule, and from mule to mule, just as if each setting had some influence on the form of the curve. If the curve is now matched against the B.T.P. diagram, the nature of that influence becomes clear. Successive ordinates of the chase-break curve correspond to successive intervals along the chase, and so to successive contour lines in the B.T.P. diagram. Consequently, if we add up the breaks along each contour line we shall obtain another frequency distribution which is essentially the same as the chase-break curve, except for the correction about duration of exposure. However, the former plots the distribution of breaks while the yarn was in the mule, whereas the latter shows them afterwards.

On comparing these two distribution curves we find a good general agreement, showing that the nip-ratching effect is not confined to those breaks which are seen to happen while the mule is running, but that it leaves scars in the yarn which reveal themselves as weak places when the yarn is put to work.

We can ascertain what proportion of the "first breaks" in each chase are due to such scars, and what proportion are due to the normal working of chance, if the two curves are suitably compared. If the breaks were due entirely to chance they should be equally common at all parts of the chase-length of yarn; in

reality this distribution will be somewhat modified by the "clamp effect" to be mentioned later (p. 215), and by the tapering distribution of twist brought about by the "mule draw effect" (p. 178); generally, however, the distribution will not be far removed from a rectangular type of curve. Now the actual form of the chase-break curve is clearly equivalent to the result of superposing the curve obtained from the B.T.P. diagram upon such a rectangular plinth or pedestal. Rough estimations made in this way would seem to indicate that fully one-third of the first breaks in mule yarn are due to scars which arose as the result of nip-ratching, even under very good spinning conditions.

The peculiar merit of the two-dimensional diagram emerges in this comparison of the two curves, for although the chase-break curve shows the same final result, it does not show the stages by which that result is reached. The B.T.P. diagram is an eminently practical and practicable method, and its intelligent use should go far towards the production of better yarns as well as of bigger output.

It is evident that when strict comparison of yarn with cotton is desired, these scar-breaks caused by nip-ratching should be eliminated. This could be done by breaking under conditions which would not allow twist to escape from the broken ends, *e.g.* over one-way plush, and then testing the halves of each chase separately. This refinement is not necessary for most purposes, since the liability to form such scars is, other things being equal, itself a function of the kind of cotton used. Probably the rigidity of the coarser hairs is an important determinant, just in the same way that a jute fibre from the bale-wrapping will cause an end to come down.

Reference has been made to the tension of the yarn, as being affected by gain and by spinning at the head. In view of the practical interest of good control over tension it seemed worth while attempting to measure the tension with exactitude, and we undertook an extensive study of the elementary physical phenomena which are displayed in such a complex form by the spinning machines. I had long thought that it should be possible to use the vibration patterns of the yarn to indicate the tension by mere observation, without in any way disturbing the yarn as it spun. The difficulties were many, because of the momentary duration of the various patterns, which alter continually as the

carriage runs out, but after having shown that the fore-and-aft was the principal vibration, one of my colleagues was able to obtain readings which completely confirmed his results obtained by using another method. This other method involved the use of a "slave-string" (p. 360) on which the tension of the yarn was balanced.

The tension was found to rise rapidly until the time at which we have seen nip-ratching to become conspicuous, after which it fell off until spinning at the head began; during this it rose rapidly to a second maximum. This second maximum in normal spinning was sometimes as high as a quarter of the single-thread strength. In the first maximum it rarely exceeded ten grams, which was quite a small fraction of the strength of the yarn at the moment; thus the direct responsibility for broken ends was again fixed upon the roller nip. The lowness of these values was rather surprising, and would scarcely have been credited if two entirely independent methods had not been available for their measurement; the usual tension on the yarn is round about three grams only, whereas a subjective judgment of the tension by finger-contact gives an impression that the tension is nearer ten times that amount; I can only ascribe the enormous discrepancy to the psychological effect of touching a "live" strand of yarn, which is not only vibrating endways through an appreciable distance, but is simultaneously rotating, with a frequency of a hundred vibrations per second or more.

This book is not intended to propound remedies for the serious fundamental defect of the mule which these studies of break-time-place and of tension have revealed. It is sufficient for my present purpose to note that it is still possible to make such a discovery as that of nip-ratching when modern technique is applied to the examination of this much-studied machine. Many mechanical methods of remedy are evidently possible, but I personally doubt whether any are desirable; the mule is already too complicated, and further compensatory improvements would only increase its complication. It works, after all, in its present form; further study would more profitably be directed much more deeply, with the object of supplanting it altogether by a more efficient machine.

The main facts about the present mule seem clear. First, that in any ordinary setting the mule is very gentle with the

yarn, never loading it up to its normal breaking-load, so long as the yarn has not been scarred by extraneous circumstances. Secondly, on the other hand, that it habitually ill-treats the sliver brutally at the moment when this is emerging from the roller nip. Incidentally it should be remembered that the breaking-load of yarn which is vibrating under the influence of the spindle tip is very different from that which obtains under the pseudo-static conditions of an ordinary dead-load test; the importance of such fore-and-aft vibrations as predominate on the mule in breaking down the yarn structure will be touched upon later (p. 228), but the behaviour of the mule is sufficiently gentle to allow a wide margin for this.

In every respect the mule is inferior to the ring-frame, except that it makes better yarn! Even in this advantage it is losing ground as the detail of ring-frame technique is improved. The reasons for its better yarn will perhaps be clearer when the discussion of yarn strength has been reached in Chap. XII, but they may here be summarised more or less as the text-books give them. First, there is the Time Factor, which in the mule is allowed ten seconds or more during which to operate upon the redistribution of twist. Secondly, there is the ostensible advantage of being able to insert only as much twist as is necessary to hold the yarn together, dragging out the thick places until they are thin enough for the redistributed twist to lock them up; we shall shortly see reason to doubt whether this is as advantageous as is commonly stated (p. 107). Thirdly, there is the direct run of the twist from spindle to roller, with only a slight taper in its distribution. Lastly, there is the violent fore-and-aft vibration of the yarn, with its resolved transverse vibration, as well as other direct transverse vibrations, and in this vibrating condition the slip and side-creep of hair on hair in the readjustment of twist and of local stress is greatly facilitated. These conditions can all be simulated in a continuous spinning-process; undoubtedly they will be thus simulated on an industrial scale before many years have elapsed; capital, floor-space, and power will then be set free for the installation of more effective drafting apparatus.

The Ring-frame.—It is only fair to confess that I misjudged the basic problem of the ring-frame for many years. It seemed eminently reasonable to think that because most of the world's spindles were ring spindles, as also because the mechanism is so

magnificently simple, therefore the inferiority of the ring-frame must be due to some subtle peculiarity in the flight of the traveller round the ring, such as was only observable with great difficulty. Although this flight looked smooth, it might well be periodic or vibratory, and such lack of smoothness could not be detected by stroboscopic methods of observation. The only direct method for examining this possibility in the flight of a traveller which makes more than a hundred revolutions per second would be provided by high-speed shadow photography of the kind applied to bullets, or to the wings of flying insects, making about ten thousand exposures per second. Research on this line was vaguely anticipated when the opportunity should arrive; but the whole presentment of the problem was suddenly inverted in a few minutes by my casual observation of the "trapped twist" phenomenon, which will be described in a later chapter (p. 179). The fact that a fixed and restricted fraction of the twisting couple could alone be effective in picking up the emergent hairs of the sliver was most significant.

At the time when I made this observation our attention had been concentrated on nip-ratching in the mule, and a colleague had extended the statistical observations to the ring-frame. By placing a piece of one-way plush just clear of the yarn, so as to catch it on breakage, we had found a most peculiar and unexpected Break-Place diagram for ring-spinning. Never did a break occur between the bobbin and the thread-guide; all were above the thread-guide, and many happened at the roller nip as we had anticipated, showing that nip-ratching was again operative. But in addition to the weak region at the roller nip it was also found that the region just above the thread guide was equally weak. This latter fact was at first surprising, but it became explicable when I realised that actual untwisting happens in this lower region as the inevitable consequence of pushing "trapped twist" upwards along the yarn.

The inferiority of the ring-frame was thus made clear; it had two weak places in its yarn, at any moment, instead of only one in the mule. It then became an open question whether the traveller and ring were to be blamed at all. To apply the "slave-string" method to measurement of yarn tension on the ring-frame was simple in comparison with our previous use of it on the mule, and another colleague made an exhaustive set of

observations, determining the tension above the thread-guide with errors of only 1% under all sorts of conditions. The details of his analysis hardly concern our general purpose in this book; [1] it is sufficient to note that a rational formula was attained, from which the tension in the yarn could be safely predicted, so long as the traveller's flight was steady; the limiting conditions under which the flight is unstable, and the tension can no longer be predicted, are reached when the bobbin is small in relation to the ring, which is the well-known difficulty of bare-spindle spinning.

The ring spindle itself was thus cleared of suspicion and shown to be a precise and dependable method for spinning and winding yarn. The thread-guide and the roller nip must bear the blame for the faults which the ring-frame possesses.

As an example of the consistent behaviour of the spindle and ring, it may be mentioned that very fair tests of the efficiency of lubricants could be made by the slave-string method. The behaviour of the traveller was also examined by the neon lamp, to ascertain its attitude during stable flight, while the transition to unstable flight could also be noted in this way; such stroboscopic methods are of little use for the investigation of this instability, and the high-speed kinematograph will still be needed for the study of this region. The condition of the surface of the ring became noticeable when we developed our methods to the use of light travellers at low speeds, so that burnishing had to be employed to give a surface good enough for steady running.

The information obtained from Mule and Ring, as well as from the more fundamental study of yarn itself, was then applied to a re-arrangement of the ring frame which is practically a new machine.

The Rule Spinner.—This rearrangement of the ring-frame is an obvious hybrid between the ring and the mule, just as the latter was itself a blend of Hargreave's jenny and Arkwright's water-frame. With such a complicated pedigree it seemed permissible to denote the rearrangement by the portmanteau word "Rule," which is, moreover, apposite because the appearance of the spinning strands of yarn does actually provide a rule by which their tension can instantly be measured.

[1] See, however, Hanton, W. A., *Mechanics of Textile Machinery*, London, 1924, p. 62.

The rearrangement was designed to achieve three separate objects simultaneously. First, time was given for the readjustment of twist, as in the mule. Secondly, the twist was made maximal at the roller nip, thus binding the emergent hairs together at once and so minimising nip-ratching. Thirdly, a shock-absorber was provided between spindle and roller by the vibration of the spinning yarn.

The method of the rearrangement was simply to increase the distance between the spindles and the rollers of a ring-frame, so that a convenient length of yarn, up to one metre, was vibrating freely between them without any thread-guide, the axis of these vibrations being the projected axis of the spindle. The rollers had thus to be brought forward over the spindle, and the slope, or "bevel," of the roller beam was exaggerated so that they fed the sliver vertically downwards, thus enabling the twist to run right into the roller nip; various intermediate stages of this slope may be adopted, or a carrier roller may be added to steer the sliver round to the nip. The rollers may be set away from the direct axial line if it is desired to impart a fore-and-aft harmonic vibration to the yarn.

The essential feature of the Rule spinner is that the distance between roller and spindle shall be sufficient for at least one node, and preferably two or more, to be present always in the vibrating yarn. The attainment of this condition is easy with fine yarns, which are spun on small rings with light travellers, and consequently with low tensions. It might be unwieldy with coarse counts, but the headroom of 50 cm. to 100 cm. required for fine counts does not make the machine awkward to handle.

The nodes and loops which form in the vibrating yarn have an effect upon the phenomenon of "ballooning" which we predicted, but were none the less pleased to verify. The ordinary ring-frame restricts ballooning by lateral preventer guides; the rule-frame does not balloon at all. When a rule spindle is started up, the yarn at once assumes a vibration pattern of definite shape and dimensions, this being determined by the size of the ring and bobbin, and by the traveller weight for any given conditions of lubrication; the position and amplitude of the nodes and loops are unaffected by any further increase of speed, because the consequent increase of tension compensates automatically for the increased frequency of the circular vibration. The maximal

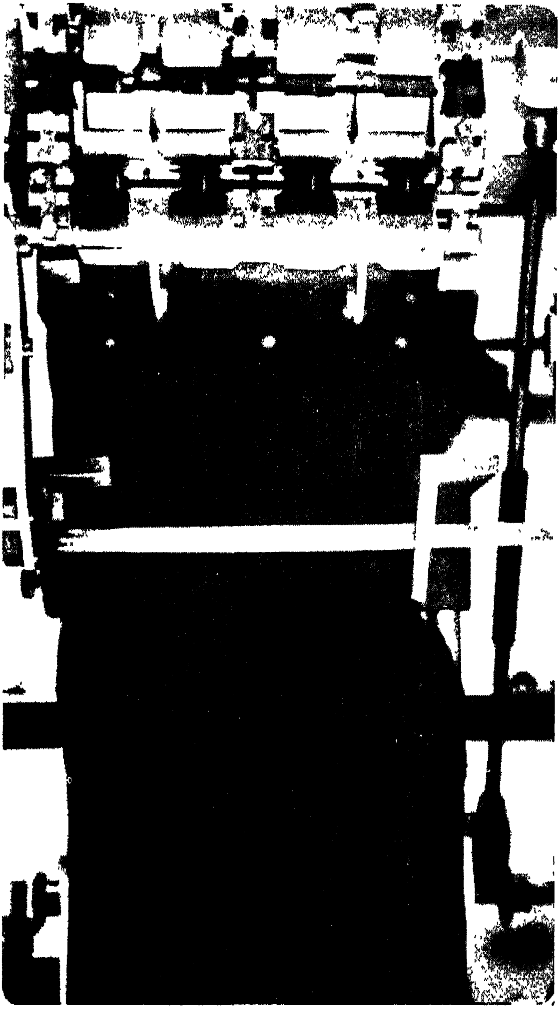

FIG. 26.—The Rule Spinning-machine. Showing four " ends " thus spinning in a modified ring-frame. Less than half the whole length of freely vibrating yarn is included. The three left-hand ends show similar " loops," their " nodes " being just below the edge of the photograph. The right-hand spindle had a lighter traveller, and its loops are shorter in consequence of the lower tension; the node at the foot of the uppermost loop, together with the beginning of the next loop, being within the photograph.

[*To face p.* 104.

amplitude of the vibration—in other words, the diameter of the "balloon"—cannot exceed the diameter of the ring; from the ring the amplitude of each successive loop decreases, first quickly, then more slowly, to the roller nip. As this applies at any and every speed, the employment of ballooning preventers is unnecessary, though an occasional partition to prevent the lashing of broken ends may be advisable in a draughty room.

The tension on any spindle can be compared with that on its neighbours by merely noting the position of the vibration nodes against horizontal parallel lines ruled behind the yarn. Roughened rings, defective lubrication, or clogging of the traveller by fly can thus be detected at a glance. The absolute tension can also be read off if the speed of the spindle (more correctly, of the traveller) be known beforehand.

The time allowed for readjustment of twist can be as long with a one-metre rule-frame as in the 1·60 metre draw of the mule; because the rule is spinning all the time, and can therefore give the same output as the mule while working at lower spindle and roller speeds. Piecing up of broken ends is done with the spindle running, as in the mule.

In the way in which it distributes the twist the Rule spinner is different from all other machines; mule, ring, throstle, or fly frame. A later chapter will return to this (p. 177), so we need only note that the twist in the Rule is at a maximum at the traveller, falls off as we pass upwards to the middle of the vibrating yarn, and then rises again to a sub-maximum at the roller nip. Thus the emergent hairs are bound up firmly at once, and are given an opportunity for readjustment later on. Moreover, they are gathered up with a sweeping action like that of a skipping-rope, and the amplitude of this sweep can be regulated, if so desired, either by regulating the number of vibration loops and so controlling the amplitude of the top loop, or by adjusting a thread-guide of any desirable aperture at any required small distance below the roller nip. The type of twisting motion at the roller nip can thus be regulated to any stage between the skipping-rope type on the one hand and simple axial rotation on the other. The speed of the twisting at the roller nip can also be thus reduced by a kind of braking action if required. The Rule spinner can thus be made into a straightforward piece of physical apparatus, in which the primary function of a spinning machine can be

regulated, and in which any accidental deviation from normal running can be detected instantly.

The shock-absorbing action of the vibrating yarn is such that most remarkable liberties can be taken with the Rule spinner. Exact concentricity of ring with spindle is an ideal which the Ring spinner tries to attain; the ring may be set a quarter of an inch off centre in the Rule, and yet spinning continues. Similarly, the plane of the traveller's path may be inclined as much as 20° to the horizontal by sloping its ring, without seriously affecting the spinning. When moderate displacements are employed, though they greatly exceed those met with in ordinary mill practice, the strength of the yarn produced is not affected, the number of broken ends is normal, and the slight asymmetry of the vibrating loops is hardly noticeable; the only obvious result of using slanting or eccentric rings is that the yarn becomes hairy, owing to the fact that it now saws backward and forward through the traveller at each revolution of such an asymmetrical spindle. The amount of yarn wound on at each revolution of a ring spindle is of the order of one millimetre only, so that quite a small eccentricity will give a backward movement of the yarn in relation to the traveller.

The speed to which a Rule spinner can be forced is very much higher than that of a ring-frame. This is due in part to the more definite initial grip of the twist on the sliver at the nip, and in part to the shock-absorbing action.

Again, it is possible to use twist-factors (Chap. X) as low as those employed on the mule, and experimental yarn has been spun on the Rule spinner at 8,000 r.p.m. with a twist factor of only 1·75.

Also, we found that under the same conditions, with ring and rule spindles working side by side in the same modified ring-frame, the number of broken ends was as great on the ring spindles at 2·5 twist factor, as on the rule spindles at 2·0.

Lastly, the yarn strengths obtained were the same on the Rule spinner as on the mule.

The ability to work in low twists was utilised to test the assumption that thick places are habitually drawn out thinner by the mule. I added a clutch to the drafting-roller drive, and spun the metre of yarn with 2·0 twist factor. The rollers were then disconnected and held out until the twist in this metre

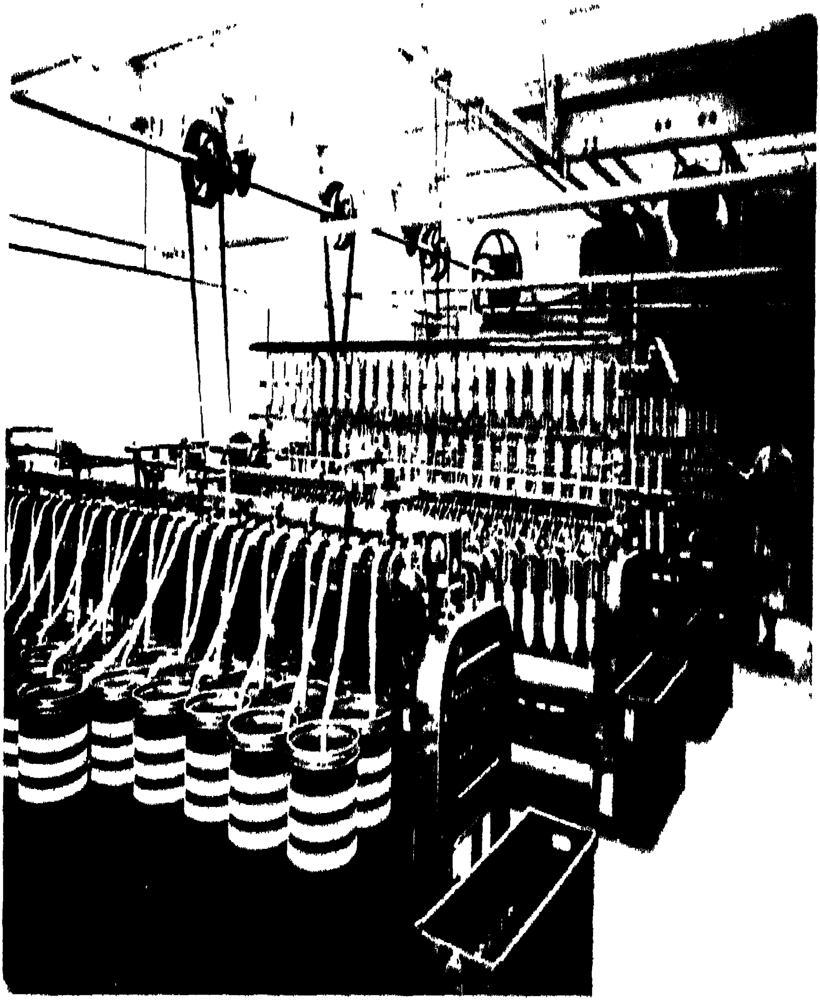

FIG. 27.—Fly-frames in the Spinning Section of the Fine Spinners' Experimental Department.

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length had reached the full twist factor. This was repeated, clutching and de-clutching by hand at the appropriate times indicated by the seconds hand of a clock, till sufficient yarn had been prepared. Lengths of less and more than a metre were also made the spinning units by altering the timing. The results invariably showed weaker yarn than by continuous spinning.

It would appear, therefore, that the semi-drafting action of "gain" in the mule may be less important than is commonly thought. Further experimental work is required. Meanwhile it is sufficient to note that the total twist can thus be varied by de-clutching, while a double-speed drive to the spindles would enable tension alterations to be made simultaneously. However, I think that if the Rule spinner is ever brought into use, the simplest possible form of it will be the most satisfactory. It is certainly a useful appliance; I have obtained normal rates of production with normal occurrence of breakages when spinning yarn so badly drafted that it would not carry a load of ten grams in single-thread half-metre tests.

It is more likely that any subsequent complication will be made by simulating the whip-snapping action of the mule spindle. It is easy to impart harmonic fore-and-aft vibrations on the Rule, whether by eccentric or tilted rings, or by placing the delivery rollers out of line with spindle axis; in this last case the yarn is not roughened. But none of these are sudden releases from tension, like that brought about by the slip of the tip-coil in the mule. On the other hand, as I was able to obtain the strength of mule yarn without any such additional devices, they may not be worth troubling about.

Fly-Frames and Draw-Frames.—While there is a great deal to be said about these machines, it all deals with their drafting performance, and will be dealt with more suitably in the next chapter. The technique of the machines in such matters as roller-covering, weighting and setting of rollers, and the prevention of ratching, is well known to the industry. It is only below the surface that we have so much to learn about these machines, on account of the predominant importance of drafting in their action. When we have learned it we shall probably be able to work out the drafts used in them by mere examination of the finished yarn. But it seems more probable that the future will see the fly-frames completely eliminated, so that the perfect

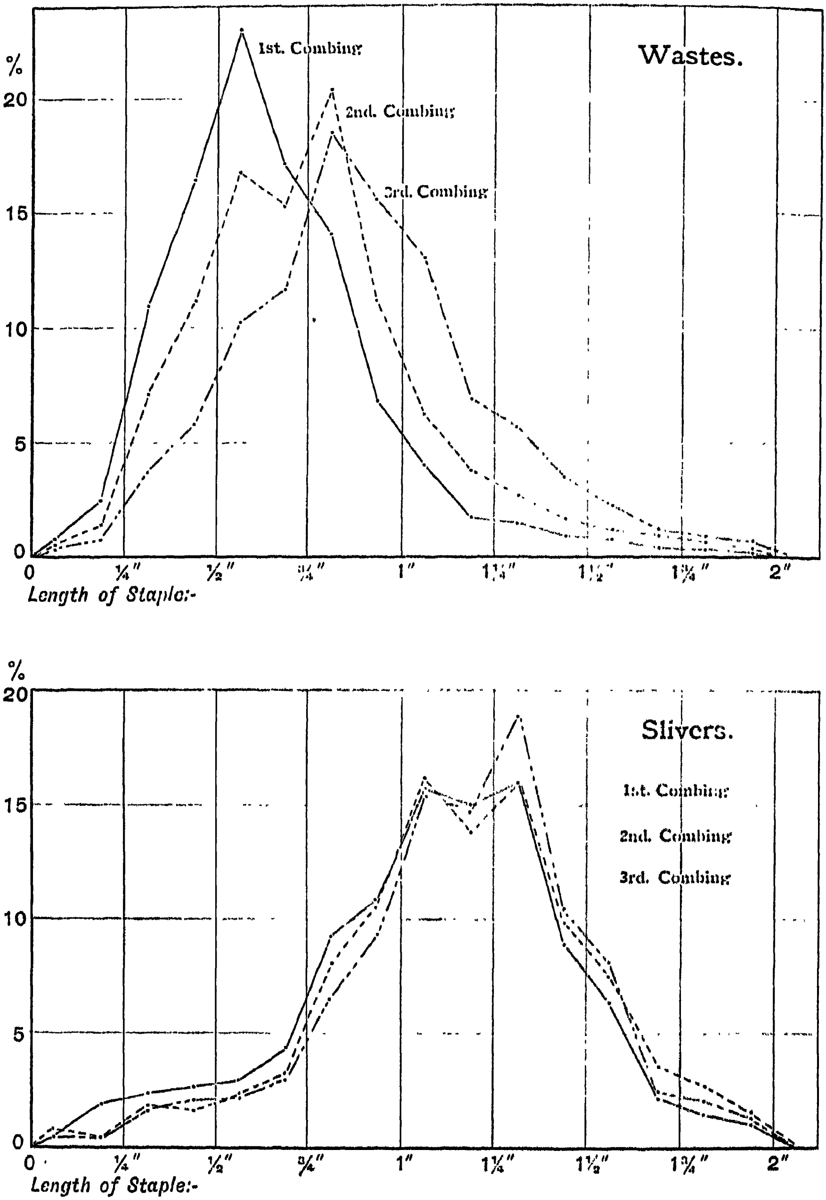

FIG. 28.—The sorting-efficiency of Combers with respect to length. Examined by sorting the wastes and slivers from triple-combing. The wastes show marked changes with each combing, but the effect on sliver-composition is slight.

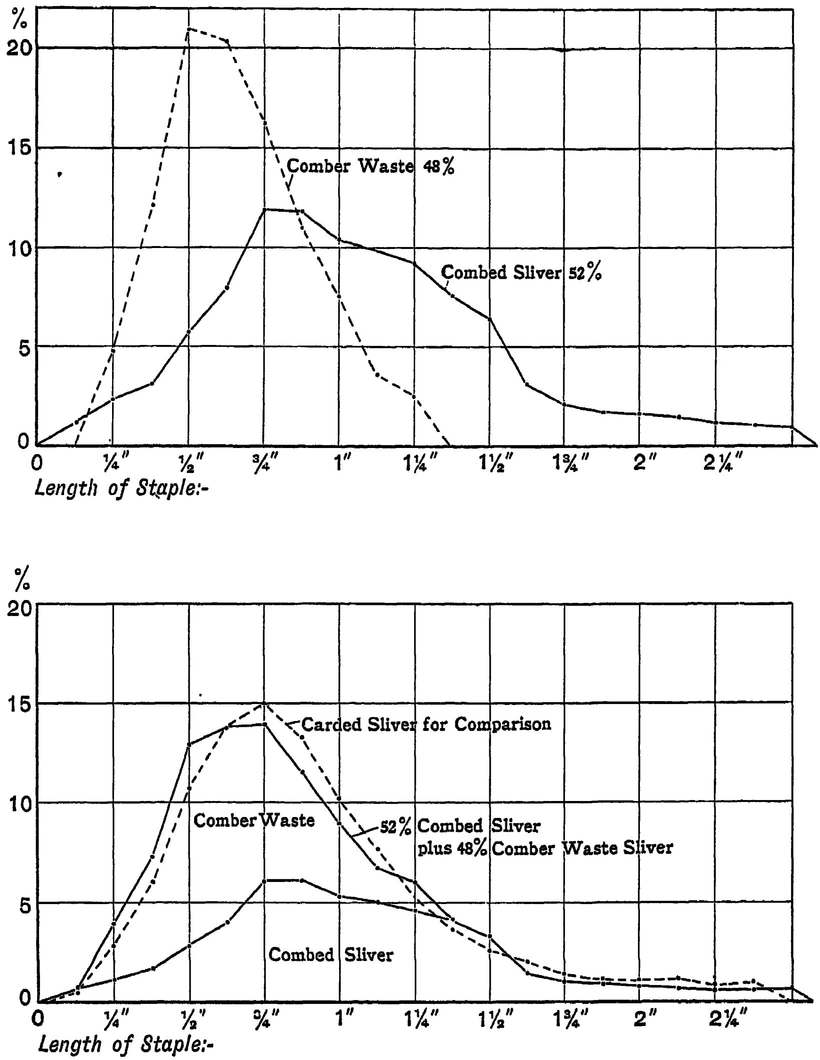

FIG. 29.—Abnormally heavy combing of cotton damaged deliberately by damp storage conditions. The length-composition of waste and silver as shown by the sorter is plotted above. In the lower diagram the correct proportions of these two are added together by plotting 48% of waste on top of 52% of sliver. The composition of the original cotton should thus be reconstituted, and this calculation is seen to be almost identical with the actual composition obtained by direct sorting of the card sliver.

sliver of the finishing draw-frame will pass direct to the spinning machine.

Combers.—When the Sorter had made it possible to study the action of the combers quantitatively (pp. 60 and 361), I found with some surprise that their chief function was not the removal of short staple. It is true that they do remove some short hairs, but these are only a fraction of the total number available for removal; usually about a quarter. Conversely, as has long been known, they remove an appreciable number of long hairs which should have been left behind. The demonstration of the former defect was not easy until the Sorter had made it possible to ascertain the length-distribution in sliver and in waste.

When such length-distributions are studied it is found that the effect of combing is barely perceptible, as between the comber lap and the comber sliver. Even triple combing still leaves the original distribution quite recognisable. I imagine that the non-recognition of this low sorting efficiency is an indirect outcome of the distorted ideas which we all used to hold with respect to "staple-length"; cotton was imagined to be fairly uniform, and so much short waste seems to be taken out by the combers that we used to think there could scarcely be any more left in the sliver.

A striking though exaggerated example can be taken from some samples damaged by bacteria, the damage being resultant upon deliberate storage in various states of wetness, which were tested for the Egyptian Cotton Research Board. In one of these the waste from a single combing was half the total weight, but even then the sorting efficiency of the comber was as low as usual.

On the other hand, I have met with cases where the length-distribution was substantially the same in two different cottons, but one of them made much more waste than the other when passed through the same comber. Therefore more factors must be implicated than the simple one of length. We know that the comber selects hairs which stick together unduly, as in the extreme case of tangled "neps," and hence it may be inferred that the surface properties of the hairs also have some influence upon this selective action of the machine; rigidity properties may also be involved. I have no exact data available in a suitable form for analysis of these possibilities, but it seems quite clear that although

the comber is a fractionating machine, it performs that fractionation on the stickiness or slipperiness of the hairs, as well as upon their length. Various minor pieces of evidence show that this fractionation is outweighed, on the whole, by its disentangling and parallelising functions, which include the removal of "nep."

Any irregularities of delivery from the comber, as in the act of "piecing," are of minor importance, as can be seen when they are expressed quantitatively in terms of single hairs, thus: suppose a comber head to be making a 0·33 hank sliver out of Sakel cotton which has a hair weight of 0·0015 mg. per cm.; this sliver from the one head will be pulled down by successive drafts of six in the comber draw-box, and in the three draw-frames, until at the finishing draw-frame it is only ten hairs thick. Any irregularities of piecing will not be conspicuous if the draftings are efficient.

The Card.—This machine seems to me the most mysterious thing in the mill, even more so than the process of drafting. The explanations I have read and heard concerning its action differ only in the degree of their improbability. My studies upon this superficially simple machine have been trivial, partly because it is less important than its successors, partly because its effects on the yarn could not be analysed until more was known about yarn, and partly because of its extreme inconvenience for direct observation.

Meanwhile it may be useful to re-state the position in terms of the single hair. A typical figure which has been quoted to me is that more than a million teeth of the cylinder wire pass over each grain of cotton delivered to the card. Putting this in a different way we find that any one hair is carded by ten thousand separate wire points; as we have seen that an ordinary hair is about two thousand calibres long, it will be struck five times in every calibre of its length. But this is incredible; it takes two to make a fight, and we may reasonably suspect that the hair dodges that fight.

A hint as to the way in which it dodges may be derived from the spinning test error which Mr. Rose and I detected and eliminated.[1] A test sample with a high hair break was put into a card which had been working on a sample with a low hair break; the card was midway between two strippings of its cylinder, this

[1] W. L. B., *Handbook of Spinning Tests*, p. 28.

having until then been the routine practice of the spinning test, with the object of obtaining normal waste percentages. The sliver was sampled at intervals from the first re-starting of the card, and the following relative hair strengths were obtained from these samples :—

Before carding, 134, 129, 138, 131, 130, 129, 132, 142; average, 132.
After carding for ½ minute, 107.
" " 2½ minutes, 119.
" " 6½ " 116.
" " 16½ " 131.
" " 28½ " 127.

These figures give clear evidence that the cotton of strength 132 which was put into the card did not come out of it immediately, and even after running for a quarter of an hour there was probably an appreciable amount of its predecessor, with strength near 100, still mixed with it in the card wire. This method might be extended to give quite accurate results by using hair weight as the measured character, but it is plainly seen that the hairs creep down into the wire and creep out again during the working of the machine.

It may reasonably be questioned whether the wire, as such, does any carding at all. It may well be the case that the hairs card one another, the wire itself serving as a retreat for clots which are too thick to be disentangled all at once. I believe that the defibrating machine invented by Mr. E. C. de Segundo for removing fuzz from seed has been found to work in this way, the seeds removing fuzz from one another by mutual friction as they whirl round inside it.

Another hint is provided by the behaviour of mercerised raw cotton, which is soft and silky, besides being changed in its electrical properties. It so chokes the wire of an ordinary card to which it is fed that the cylinder soon refuses to rotate. I am indebted to a colleague's experiments for this incidental information.

Another hint is given by the card's performance of a limited amount of fractionation, not only with respect to length, but also with respect to fineness. It was noticed after carding some cotton that the hair weight of the sliver was a little less than that of the strippings from flats and cylinder. These strippings were returned to the card, and again the hair weight of the new strippings was raised about 2% above that of the new sliver, making a

total shift of 4% above that of the original cotton. Now although the experiment was incidental, and the actual shift very small, it is a result of some importance, in that it proves the possibility of sifting out coarse hairs from fine ones; it may be that serious study of the card will accidentally provide a clue to the method by which this may be done.

While the thoroughness of carding is necessarily important, as affecting the freedom of hair movement in subsequent drafting, it would seem to me that the regularity of the sliver is unimportant. Its irregularities have no chance of survival at the finishing draw-frame, for the individual card slivers of even a single combed preparation will have been attenuated at that stage to a thickness of only one-tenth of a single hair; that is to say, each hair will be separated from any other hair derived from the same card sliver by a distance which will average nine hair lengths. This bald fact serves at the same time to illustrate the importance of thorough disentanglement by the card and by the comber.

I have been informed, though it happens that I have not personally verified the information, that the card sliver irregularities occur in regular waves, many yards in length, as if the same pseudo-periodic error which we shall presently discuss in Drafting was also operating in the exaggerated " draft " exercised by the card. Indeed, if the hairs do actually card one another, this action would be identical in character with that which causes the " hair displacement error " in drafting and sorting, and the formation of long waves in the card sliver might follow as a statistical consequence. An interesting demonstration of the existence of such waves and, at the same time, of their unimportance to the final spinning, was provided by the very beautiful patent of Mr. W. Hamer. He abolished these waves by passing the sliver through conical drafting rollers, across which the sliver was traversed automatically by its own variations in thickness, with consequent alteration of the draft; I am informed that it worked perfectly, but made no difference whatever to the finished yarn.

THE CHANGES PRODUCED BY THE MACHINES UPON THE COTTON

For our purpose of studying the relationship between cotton and yarn it is now necessary to glance at some indications which show how far the original properties of the raw cotton are likely to

be distorted by passage through the machines. For this purpose there is no difference in importance between one machine and another, since any damage to the hair by bruising, breakage, or knotting will either cause that hair to be eliminated altogether or will leave it modified. This is consequently a different point of view from that which we have occupied so far, where the importance of each machine in its effect on the construction of the yarn has necessarily been directly proportional to its distance from the act of spinning.

It is hardly worth while to attempt in these pages to deal at any length with such reactions of the cotton to its treatment. The technique for study of the hair (Chap. V) is coming into general use, and its application to the examination of samples drawn from various stages of preparation and spinning, with various settings of the machinery, in mills which are worked with varying degrees of care and skill, is relatively a simple procedure. The next few years will probably see a thorough exploration of this ground, so that it will suffice to describe here a few typical examples from my own survey of mill machines working under normal conditions, without attempting to particularise details which are peculiar to individual mills, but may be described as representing the best current practice.

Change in the Slip Spiral.—The number of slip spirals found in a given length of hair changed roughly as follows:

Original ginned lint	2	
Card sliver	4	
Comber lap	4	
First comber waste		6
First comber sliver	4	
Second comber sliver	6	

Ginning and combing would seem to be chiefly responsible for slip spiral formation. There appears to be no further change till the yarn is reached.

Change in the Neps.—The determination of these artifacts (pp. 19 and 52) by counting the number present in samples weighing 0·1 gram is extremely tedious, but quite practicable. A typical set of data was as follows, the numbers representing neps per gram.

Original lint	515	
Card sliver	755	
Card strips		3,080
Comber lap	688	
First comber waste		1,772
First comber sliver	109	
Second comber waste		676
Second comber sliver	93	
Finishing draw-frame	152	

Computing these figures further on the data available for waste, we find that the card raised the nep content from 500 to 1,000 per gram, but put away a quarter of this thousand in the waste. On the other hand, the combers did not alter the total nep content; the first comber put aside 80% of the neps into its waste, while the second comber only disposed of 30% of the remainder. The last figure indicates that drafting makes a little nep, which is not unlikely.

Bearing in mind the fact that there is no nep in the unopened boll, these figures show very well how we make trouble for ourselves in the mill. The nep-making proclivities of the card are, of course, well known, though I do not think that its inefficiency as a nep-remover is fully realised. Any similar rubbing action will make nep; in one arrangement accidentally produced during work on the Sorter we converted almost the whole of the cotton which passed through it into nep.

Change in Hair Break.—It has generally been assumed that the blowing-room machinery, in particular, has a weakening effect upon the hairs. We have just seen that there is a treble increase in the number of slip spirals, but the presence of these has yet to be correlated definitely with any depreciation of hair strength. Further, we shall see later that a roughening of the surface of the hair might weaken the yarn produced (Chaps. VIII and XII), and such an effect might be confused with weakening of the hair itself.

If we confine our attention to the actual strength of the hair we obtain typical data which are negative, and show no significant indication of any weakening by any of the machines. The following data were taken from the spinnings described in the *Handbook of Spinning Tests* (p. 37), whereby the accuracy of the

method was examined upon duplicated samples. The breaking load is given in arbitrary units for each group of fifty hairs tested.

Original ginned lint .	134	130	138			
Scutcher lap . .	131	130	129	142	132	
Card sliver (v.s.) . .	(107	119	116)		131	127
Finishing draw-frame sliver	141					
Slubber roving . .	132					
Intermediate roving .	133					

The absence of weakening effects in the scutcher has since been independently demonstrated by the Shirley Institute. The incidence of the dilution errors (shown in brackets) by the hairs remaining in the cylinder wire has already been mentioned.

Changes in Length.—Alterations of the length of the hairs might be effected either by breakage or by stretching. The latter could be detected by critical use of the hair-weight method in conjunction with careful sorting. As regards breakage, which is easily detectable by sorting if extreme care be taken in preparing the sorter slivers to make them equally perfect from all stages of the sampling, there is no striking change in the length composition of the sample after scutching.

The mass and speed of the beater blades in the scutcher look more impressive than they are in reality, presumably for the same reason as that suggested with respect to carding—that the cotton does not stand up to be hit. The action of the roller gin would be expected to show more traces of brutality than the scutcher, but samples provided by Mr. H. C. Jefferys from the ginnery of the State Domains of Egypt showed only a very slight difference between seed-cotton and lint, which may well be covered by the sampling error of such a difficult comparison. Scutchers and gins can undoubtedly be so set and worked as to damage and break the cotton, but it is clear that under the conditions of a Fine Spinners' mill or the Domains ginnery the injury is very slight.

The small extent to which combing is selective for length has already been illustrated (p. 108). The addition of waste to sliver was there shown to restore the original composition of the comber lap, a result which could not be obtained if breakage in the comber were appreciable.

I therefore conclude that the clumsy machinery of cotton-spinning, so long as it is skilfully worked, ill-treats the cotton to an extent which is surprisingly small, and less than common opinion holds to be the case. The cotton which comes out as yarn is very much the same as the cotton which went into the bale-breaker, except in its cleanliness and arrangement. The general importance of this broad conclusion—which is, of course, far from being true in particular cases—lies in the fact that the cotton-breeder and cotton-grower can be confident that any improvement they may bring about in the raw material will find its way through to the improvement of the finished yarn.

THE MACHINES OF THE FUTURE

Though speculation is not relevant to the immediate purpose of this chapter, I may perhaps be pardoned by the reader for a page or two devoted to a tempting digression. The industry of cotton-growing and cotton-spinning is entering upon a transition stage, due in part to the development of competing raw materials (Chap. XVII), and in part to the discoveries of research workers both inside and outside the industry. Some changes in the equipment of the spinning mill seem to be indicated as a future consequence of this transition.

To begin with those minor details upon which industrial success or failure depends, in the long run, I feel sure that much could be done by the wedding of scientific instrument design to textile machinery making. Either of these branches of engineering stands apart from standard engineering practice. Textile machinery must be inexpensive, because so much of it is needed; it must be accurate because it is handling single cotton hairs. The desirability of a hybrid is evident. I have noted incidentally one or two really impossible forms of present construction, such as the squared roller joints, the lengthened box of the mule carriage, and conical holes in skewer footsteps, but many others call for revision; it should be impossible to put top and bottom rollers out of parallel when they are being set, and the method of setting roller-intervals with spanner and feeler gauge along the whole length of a mule is a relic from an epoch of specialised production in which re-setting was hardly ever needed. In an experimental spinning mill these crude methods of adjustment bring about a preposterous waste of time and of skill.

Many of the constructional details are now undergoing revision, and the availability of new constructional materials, such as duralumin and stainless steel, will provide not only the opportunity but also the necessity for still more thorough revision, and for the creation of machines which will be less strikingly disproportioned to the work they are doing.

The greatest opportunity for the development of a function would seem to be presented to us by fractionation. It is strange that nobody, so far as I am aware, has yet commented upon the fact that although the spinning of flax and worsted are less highly specialised than cotton-spinning, yet both of them find it worth while to employ skilled operatives upon the task of fractionating their raw material, so that the machines are fed with tolerably homogeneous fibre. Exactly the reverse holds good in cotton-spinning; the difficulty of such fractionation has been accepted as insuperable, and the same constancy has been obtained in the food of the machines by the reverse operation of mixing. This act of mixing brings about uniformity from month to month, as the statistical consequence of skilful mixing, though it necessarily increases the heterogeneity of the cotton spun.

The latter consequence did not seem obvious so long as we thought about cotton as a "material." Having come to look upon it as a mass of immensely variable single hairs, we can see that inability to fractionate it is cramping our style, and that great possibilities are awaiting development when we can classify our raw material before using it, just as the flax and worsted spinners classify theirs. Any sample of cotton contains a wider range of coarse and fine hairs than the range of the average of the whole world's raw cotton; any sample contains a wide range of hair lengths (Chap. IV). We shall see in later chapters some reason to think that fractionation of raw cotton into its constituent groups might already be commercially possible with respect to hair length, and that the far more important method for fractionation with respect to fineness is merely waiting for its discoverer (Chap. IX). Therefore it seems to me that the great change of the future will consist in the formation of a new department, or even of a new industry preceding the present spinning mill, where the complex mixture of hairs which exists in the purest raw material will be split up into more homogeneous

classes. The variability from hair to hair on the single seed sets the upper limit of improvement to which the grower can go in this respect; we have seen already how great that variability is, and it is evident that the spinner cannot get a very great deal more uniformity from the grower than he is getting already (Chap. XVI). Further improvements in this direction must be made by the spinner himself.

Whether such fractionation is worth doing is, of course, almost beyond prediction, for it depends upon too many unknown economic quantities. While we shall see reason to doubt its profitability in the matter of length, there seems less doubt about fineness (p. 163); by the analogy of the comber on the one hand, and of the complicated mule on the other, we can be sure that fractionation would have to be very slow and very difficult in order to prevent its general adoption. Some of its cost would be saved by its ability to work upon badly-cultivated crops, grown from mixed seed, just as well as upon the best cottons available. From the data collected in this book I think we are entitled to a rough inference that fractionation for fineness would be worth while, and that the reduced output per unit of capital might be balanced by other modifications in the machines and in their labour requirements, such as the substitution of a continuous spinner for the mule, and the elimination of the fly-frame by the use of enormous drafts (p. 137).

An improved uniformity of the raw material would simplify the manipulation of the hairs under such drafts, and the facts to be discussed in the next chapter make it fairly probable that the future may see the introduction of a micrometer type of draw-box which will achieve that exact control of the hairs to which the modest high-draft systems of the last decade are feeling their way.

Continuous spinning, as distinct from the intermittent action of the mule, is to me a foregone conclusion. We shall presently see that most of the obstacles in the way of producing yarn according to prescription are located in the drafting process. The assistance of the mule in partially rectifying errors of drafting will therefore be so much the less necessary in the future.

These speculations carry the implicit assumption that cotton-spinning will remain a purely mechanical process as it is at present. To speculate beyond this assumption would be

altogether unprofitable, but I must confess to an uneasy suspicion that some non-mechanical method is lying in wait for some inventor who is not so familiar with the existing methods as to be prevented from seeing something otherwise obvious. That misbehaviour of cotton hairs which happens in an electrical field of force is one usable phenomenon which has not yet been harnessed to our service.

As in the rest of this book, I have assumed that cotton-spinning in the future will be preoccupied with quality rather than with output, because its strength is its last line of defence against the artificial fibres. This in spite of the fact that the daily Press reports speeches which explain how a few strands of artificial silk in a cheap cotton fabric will make it so beautiful that the cotton exports will increase enormously. This is a pleasant optimism, but scarcely reasonable.

CHAPTER VIII

DRAFTING

In the first chapter of this Part I have indicated the general relation of the drafting phenomena to our subject (p. 81). I quoted from a text-book taken at random to show the nature of accepted opinion, and then asserted that drafting, far from improving the uniformity of the sliver, was fundamentally responsible for the way in which actual yarn deviates from the cylindrical form of the ideal. The ingenuity of the machines, which have meanwhile been examined, tends to disguise the fact that in one way or another, directly or indirectly, they are the servants of the drafting rollers; drafting is the essential operation in all such short-staple spinning as produces cotton, flax, worsted, or waste silk yarns. I have therefore evaded the subject in the previous chapter in order to deal with it separately.

From the list of functions of the machines (p. 90) we saw that the drafting rollers play a part in mixing, disentanglement, and parallelisation, as also in the equalisation and in the reduction of number. Further consideration of this list shows that mixing and equalisation are accessory functions, the reduction of number providing the spinner with an opportunity to effect them conveniently. Moreover, since this reduction of number is effected by dragging one hair forward, past another which is held back, the functions of disentanglement and parallelisation follow automatically. Thus the primary function of the drafting process, as performed by the present rollers, is the Reduction of Number. Our present examination of this function is concerned with finding out how far it is smoothly performed; if that reduction be not smooth and predictable, to what causes may the imperfections be attributed? Having obtained some outline knowledge of the degree and extent of these imperfections, we shall then be in a position to apply it to analysing the strength of yarn.

Current Practice

The art of manipulating cotton with finger and thumb in the hand-spinning process, so as to feed a continuous thin sliver into the yarn which the primitive spindle is twisting, is well worth the trouble of acquiring. However good the original sliver may be, it persists in flowing unevenly, and the formation of thick and thin places is seen and felt to be almost independent of the action of the fingers. The intelligence of the hand-spinner is largely devoted to correcting unceasingly this tendency of the cotton to form clots. Even an expert hand spinner, much more an inexpert one like myself, would be surprised to learn that the prevalent opinion of machine spinners blames the machinery, not the cotton, for this tendency. It will presently be convenient to refer to this alternation of thick and thin places, or clotting, or beading, by the designation of "The Drafting Wave."

When the drafting rollers were invented by Wyatt, and patented by Paul in 1738,[1] the textile industries were revolutionised. These rollers are such an integral part of all the principal textile machinery of to-day, they act so smoothly, and that action is ostensibly so simple, that the fundamental importance of Wyatt's invention is apt to be overlooked. It is only by actual experience of hand-spinning that we can realise how impossible it must have seemed to Wyatt's contemporaries that there should be any practicable substitute for fingers and thumbs. Even if the idea of using differential roller speeds had presented itself to a hand-spinner, he would scarcely have troubled to make the trial upon such an impracticable material as cotton. It seems to me that Wyatt is a far greater pioneer than Hargreaves, Arkwright, or Crompton; if it should eventually be shown that the lengths of the drafting waves are a definite peculiarity of any given sample of cotton, the name of a "wyatt" should be given to this fundamental measure.

A great deal of technical knowledge has accumulated as the result of this successful manipulation of cotton by rollers. Some of it is experimental, some traditional; its details do not concern us. It deals with the substance, form, pivoting, covering and varnishing of the rollers. It may be classified into groups which

[1] Barnes, E., *History of the Cotton Manufacture*, London, 1834.

deal with the clamping efficiency of the roller nip, with the phenomena of "licking," and with durability. All of them aim at securing an unimpeded flow of cotton through the rollers. The clamping has to be made by a line contact between two rollers, holding the hairs without bruising against the resistance of their fellows; as this is impracticable unless one roller is partly deformable, such details as the depth and kind of pattern on the bottom roller, the weighting and deformability of the top roller covering, the friction of its pivots, and the coefficient of friction of its surface are all implicated. The act of "licking" may be due to mechanical roughness, to moisture condensation, or to electrification; its prevention concerns only the surface of the rollers, in relation to the environmental conditions. The durability group of details is concerned largely with the surface, but also with the deformable covering and its foundations, and with the bearings; convenience of manipulation may be included in this group. For our present purpose the reader can safely assume that all this technique is now fully competent to its purpose, and that whatever may have been the trouble of the early spinners, the act of drafting can be performed in a good modern mill as smoothly as is mechanically practicable. Relatively simple tests can be made which show that there is no synchronism in such a mill between the operation of any of these details and the appearance of drafting waves.

A more fundamental set of considerations deals with the choice of suitable temperatures and humidities, with the setting of the rollers to the most smoothly-working interval, and with the effect of roller-weighting on the latter. Some light has been thrown on the first of these subjects by the work of a colleague, already described (pp. 31 and 33), in measuring the conductivity and flexibility of the hair. The others are the subject of much traditional knowledge, but when such a varied assortment of cottons as pass through an experimental mill are being spun, the setting of the rollers comes down to a process of trial and error. In the next chapter we shall notice incidentally that the connection between roller setting and staple length is much less sharply defined than the text-books would affirm.

The accumulation of this knowledge of details enables the modern mill to draft laps of cotton ten inches in width, at speeds which would have astonished Wyatt himself. Nevertheless, we

have scarcely progressed at all in our understanding of the process in its action upon the cotton by itself, when all accessory defects of the roller appliance have been eliminated. There would seem to be no doubt that the defects of the early rollers had a masking effect on the essential peculiarities of the reaction between roller and cotton; these defects have long been eliminated, but a traditional belief in the perfect smoothness of drafting (p. 82) has prevented people from recognising this, and roller defects are still being sought when none of importance exist. Such imperfections as the drafting process now exhibits must be ascribed to the cotton itself.

Before dealing with these imperfections it is rather important to notice that during the present century there has begun a steady change in the practice of drafting, which change is as yet very far from completion. The early roller arrangements were designed to hold their victims with a firm grip, and the closest roller setting was supposed to be limited by the length of staple; closer settings would break the long hairs. We have seen, however, that staple length is rather indefinable (pp. 10 and 60), and experience with the hair-break test (p. 353) provides some hints that it is very difficult to hold a cotton hair firmly; it is primarily due to these two facts that of late years the setting and weighting of rollers have been modified so that the thinner rovings are now held at less than staple length, and without a definite grip, the hairs being allowed to hold one another. Even in the early days of cotton-spinning, of course, it had been recognised that thick slivers needed a wider roller setting than thin ones, the minimum of staple length being considerably exceeded in the former. This is due to the zone of compressional stress extending further from the roller nip in thick slivers, so that the mutual support of hair by hair still gives an adequate grip at some distance from the nip.

So long as only a few hairs are grasped at one time it is possible to drag them out of a roller nip which seems quite firm. To emphasise this we may bear in mind that a single hair is rarely big enough to fill up a groove (such as would be made by a scratch) which is only one-thousandth of an inch in depth. The most uniform and smoothly acting grip which we can take upon a cotton hair is obtained as a kind of statistical average, when the crests of its convolutions are in contact at dozens of points with

those of a mass of other hairs in which it is embedded. Thus it was found conducive to smoother drafting and better production to close up the roller settings and to lessen the weighting of the top middle roller; this was done at first in the mule, and then in the preceding fly-frames. The amount of drafting is settled by the firmer grip of the front and back rollers, while the middle rollers merely keep the sliver compressed, so that the sliver itself acts as a supporting matrix for those hairs which are waiting their turn to be gripped and drawn forward by the front rollers. Considerations of simple geometry show that the amount of support will vary from one part of the cross-section of the sliver to another; this cross-section is ovoid, compressed between two parallel rollers, so that the pressure is at a maximum in the middle and least at the ends of the oval; the twisting of the roving serves an accessory purpose in minimising this defect.

Various inventions have been patented during recent years which are logical extensions of this modified practice. They happen to have a particular interest for me, because I had started to study the drafting problem before the first of them came to my notice; it was reassuring then to find that they were practically identical with experimental devices which I had already tried, and discarded for the reason that they were partial improvements only, not cures. The Casablanca method feeds the roving between two leather belts, thus giving a longer zone of mutual support, and carries such support rather closer to the roller nip than would be practicable with rollers of workable size; it is thus a simple extension of the close setting already practised in fine spinning. The Jenninck patents act similarly by using additional middle rollers and light top rollers. In both these methods, as in the more usual practice, simple geometrical facts make it impossible to support the hairs adequately by the use of rollers alone; practical reasons prevent the diameter of the front delivery roller from being less than half an inch, and even if it were workable at a diameter of one-eighth of an inch, there would still be a quarter-inch gap between the two roller nips.

In one patent by Jenny this gap is much reduced by the use of plates instead of middle rollers. It could be further reduced by abolishing rollers altogether, replacing them by reciprocating nippers; it could be made very small by using single rollers and "doctor knives," as in the McCarthy gin. Such suggestions

appear fantastic, but it will suffice here to note that the reduction of this gap is very important.

Having sketched the principal features of the drafting appliances used in current practice, attention can now be directed to the way in which the cotton reacts to these appliances. It is understood that adventitious defects in the appliances are excluded from discussion, and that we are dealing with fundamental peculiarities of the cotton itself.

Observational Evidence

It will be necessary for the reader to discriminate sharply between the observation of facts and their interpretation. The observed facts show indisputably that the very best drafting technique is unable to generate a uniform sliver. They also show that the effects of each drafting can be traced through to the finished yarn. Thus far my studies have been successful. There has been no corresponding success in analysing, quantitatively, the causes which control the formation of these drafting waves, though qualitative suggestions will be made in a later section, after some experimental evidence has been reviewed. For the present section we are only concerned with the facts observed.

These observations were first begun on the yarns of the Thirteen Samples, while control observations were taken at the same time upon yarns made for aeroplane fabric. The latter yarns were as nearly perfect as the former were imperfect, but the facts observed were only different in degree, not in character. The complex pedigree of foldings of roving upon roving, in the history of yarn which has been spun in the usual way, is productive of such complicated structure that it will be less difficult for the reader to appreciate these facts if they are taken from some yarn which was specially spun at a much later date, in such a way as to avoid any folding whatever.

The finishing draw-frame sliver is the natural starting-point for investigations on drafting because of its uniformity. The statistical basis of this uniformity can be expressed in a simple manner, but this is more conveniently considered in a later chapter (p. 205), when discussing the parallel case of two-folding in yarn. For the present we need only note that it is admittedly the most uniform arrangement of cotton hairs which the spinning mill can show, and that this uniformity is a statistical consequence

of its laminated structure. The delicacy of this lamination is best appreciated by expressing it in terms of single hairs; such a sliver as was quoted with reference to the comber piecings (p. 111) will consist of 12,000 hairs in cross-section at any given place. In passing through the three draw-frames, if a draft of six be employed in each, it will be reduced by the first to a thickness of 2,000 hairs; these are superposed in six laminæ for the second frame, but each separate lamina is thereby reduced to an average thickness of 333 hairs, while the finishing frame will reduce it to a mere 55 hairs. The lamination will go a stage beyond even this in fine spinning, because the comber draw-box precedes the regular draw-frames, and the original slivers from each head of the comber are drawn down to some ten hairs only. The finishing draw-frame sliver of a combed preparation may thus be regarded as consisting of more than a thousand separate laminæ, each with its own independent drafting history. Under the conditions which obtain in a well-run mill it would appear from statistical evidence that a very fair approximation to such independence is actually achieved, the drafting of each lamina taking place without much effect on its neighbours, so long as it comprises more than a hundred hairs in cross-section. In so far as the previous preparation has left the hairs tangled, interlacing, or crossed, so the removal of a hair from one lamina will disturb those in adjacent laminæ, the drafting of each lamina will not be independent, and the full statistical consequences will not be reached. Given good preparation we may regard the finishing draw-frame sliver as being practically homogeneous, so that if it is cut up into short lengths it will show no appreciable variation of weight which is not covered by the experimental errors of such cutting.

The special yarn to be described was made by drafting such a sliver in the slubber, passing the slubber roving to the intermediate frame without folding two ends together, and again passing this single end to its final draft in the spinning machine. Samples drawn from each stage were cut up into lengths of 1 cm. and these were separately weighed. The weights obtained were plotted in sequence to show the consecutive variation of weight along the two rovings and along the yarn. Study of graphs obtained in this way then shows the following general features:

The Slubber roving is no longer uniform, as compared with the finishing draw-frame sliver from which it was drafted. The curve which shows its sequential variation of weight is slightly beaded, there being a regular recurrence of thicker and thinner places, as if the drafting had made it irregular to a slight extent, but in an orderly manner. An analogy at once suggests itself with the corrugation of road surfaces and of tramway rails, with the merely relative difference that in drafting the "road" is moving while the "wheel" is fixed.

By the Intermediate frame, at the next drafting, these waves are necessarily attenuated in proportion to the amount of draft employed in this frame. Their crests are separated widely. If we can recognise the distance from crest to crest, or wave-length, with sufficient accuracy to measure the new wave length as well as the old one, we have measured the amount of the draft. The draft is simply the ratio of the two measurements.

This measurement is not too easy, even in this simple case, because the new drafting has generated fresh waves of variation in thickness. These seem to be of the same length as those which were made in the slubber, but they are of greater amplitude, and they are superposed upon the now attenuated slubber-made waves.

At the Spinning frame the same two processes are repeated. There is simple attenuation of the pre-existing waves made in the slubber and in the intermediate, the former now becoming very long indeed. There is also a further generation of new waves, superposed upon the attenuated pre-existing ones, and these new waves are of still greater amplitude—relatively to the decreased weight of the yarn—than they were in the intermediate frame, just as these were more noticeable than those made in the slubber. In all three machines the length of the newly generated waves would appear to be at least broadly similar.

With these simple facts before us we are at once in a position to settle the nature of the principal determining factor in yarn irregularity, which has sometimes been ascribed to the actual spinning, or to the twist. The waves of thick and thin places are shown to be generated in just the same way, though with increasing intensity, by rovings as well as by yarn. The drafting process must act selectively upon either the weight of the roving,

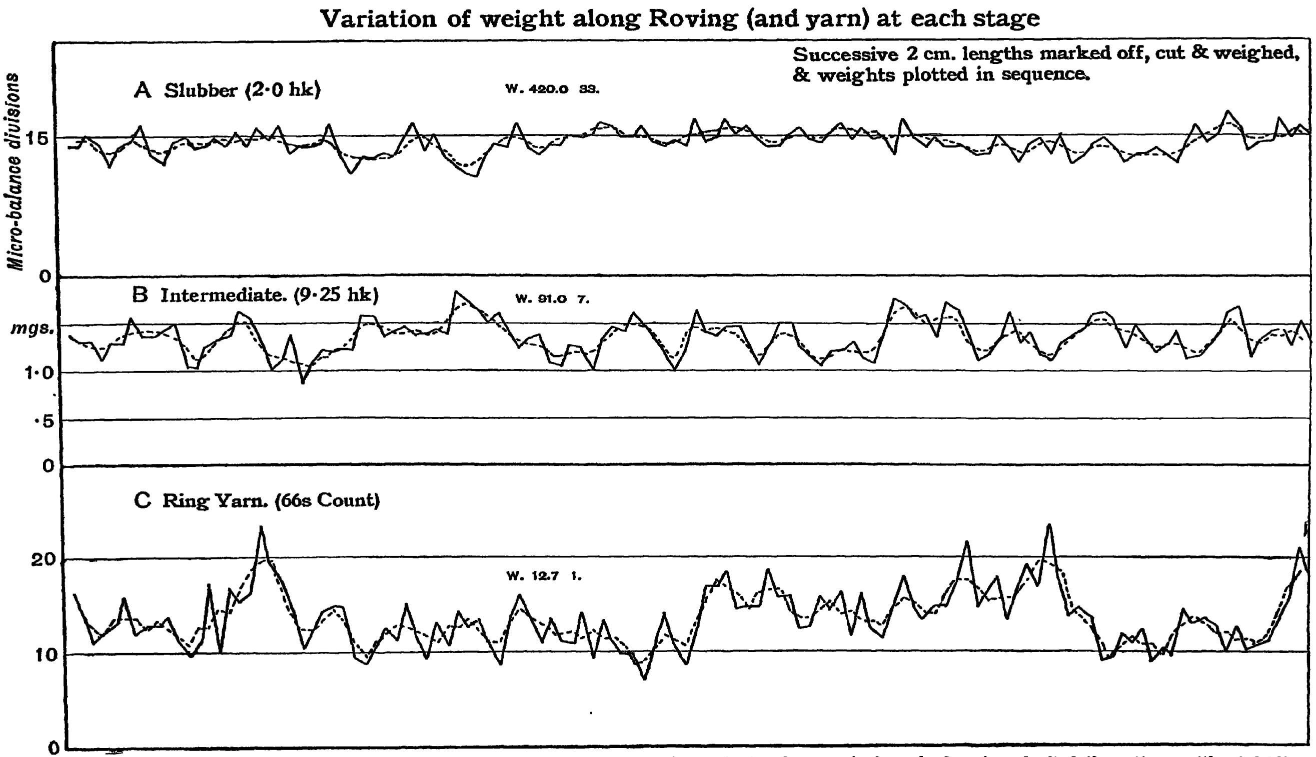

FIG. 30.—The formation and attenuation of Drafting Waves as shown when spinning from a single end of roving, drafted three times without folding. Each graph represents a single length of 240 cm. of roving or yarn, taken at random from each machine. This length was cut up into 2-cm. lengths, each of which was weighed, and the weighings are plotted in sequence. A smooth curve of consecutive five-point means is drawn through the actual weighings. The uniform finishing draw-frame sliver emerged from the slubber with uneven short waves in it, as shown at A. A further draft in the intermediate frame produced B, in which these waves are elongated, and new short ones appear. The final draft of 7 produces yarn which shows, in C, still more complex wave-forms, with still greater relative amplitude.

upon its size, or upon the number of hairs it contains. Later we shall see reason to conclude that the primary factor is the number of hairs, so that the variations shown by weight, diameter, cross-section, and twist in yarn or in roving are all consequent upon the failure of the drafting process to deal uniformly with the number of hairs presented to it.

The Amplitude of the waves is very noticeable. It would seem to become greater, relatively to the average size of the roving, as the roving becomes smaller. Whatever causes may be operative in producing these waves would seem to be more influential in thin layers than in thick ones; this suggests that we shall eventually find a statistical basis for these causes, and harmonises with the view that the number of hair units is the primary factor. Here we come upon an important practical implication, that cotton might be spun to much finer yarns than are at present practicable; our present limits are reached because the increasing amplitude of these waves drives a wedge, so to speak, right through the sliver at a stage of average fineness when there would otherwise be no difficulty in handling the yarn. Apart from experimental spinnings in the Experimental Department, only two mills in the world habitually produce counts finer than 250s, so far as I am aware; if the amplitude of these waves could be reduced, such counts could easily be exceeded.

The magnitude of these wave-like irregularities in the yarn is best realised by comparing them with the "mule-draw effect" wave (p. 178). This latter seemed a very important variation and defect of mule yarn when it was first discovered, but it is so insignificant beside the drafting irregularities that some twenty mule chases must be averaged out before it can be clearly seen among the so-called "chance variations" due to the drafting. The more important nip-ratching defect (p. 94) is also insignificant beside them, but in a different manner; its amplitude may be greater than that of the drafting wave—we have seen that it may cut clear through the spinning yarn—but it occurs intermittently and casually, whereas the drafting wave is continuous, and its principal form is only a few centimetres long when first generated.

Yet these secondary distortions of the drafting wave are important to the student. In my early observations I turned to ring-spun yarn in the hope of obtaining a yarn structure free

from those distortions which the gain of the mule was then believed to impose. No improvement in the exhibition of the phenomena was found, and we now know that this was because ring yarn is even more distorted by nip-ratching and guide-ratching than mule yarn. Now although such secondary distortions as are produced by the mule-draw effect (upon diameter), by nip-ratching, and by gain also, have an insignificant effect upon yarn structure in comparison with the effects of drafting, yet their presence makes the exact study of undiluted drafting phenomena in yarn very difficult, especially when periodicities are being sought. Quite small local distortions imposed on the drafting wave after the sliver has left the rollers are sufficient to baffle any attempt at statistical analysis of the exact form of the wave as shown in yarn. And it is in yarn that the amplitude of the drafting wave is greatest, and its importance most direct.

Some evasion of these difficulties can be made by using the jack-frame as a spinning machine and avoiding these accessory distortions, but this limits the study to the less interesting coarse yarns. Smoother results can thus be obtained, as we have already seen when spinning single ends, but for my present purpose it is more directly useful to observe the phenomena presented by ordinary yarns.

My first clue to the significance of drafting was obtained by noticing that an automatic (p. 348) record of the strength of yarn, which was being tested in consecutive lengths along a single cop, showed a wave-like grouping of alternately weak and strong breaks. These will be discussed in a later chapter; for the moment it need only be noted that they were due to variations in yarn weight, of the same kind which we have just noted in the special single-end spinning. However, in these ordinary yarns it is obvious that the folding of eight slubber rovings, in successive pairs of laminæ through the subsequent machines, must have the result of complicating the final result still further. No doubt the variations of yarn diameter (hair number) do in themselves provide a hieroglyphic record of the passage of the cotton through the machines, but we do not yet know enough to be able to decipher them. Single-end spinning on the jack-frame may eventually serve as our Rosetta Stone; we shall then be able to deduce the drafts used in each machine by mere examination of the yarn.

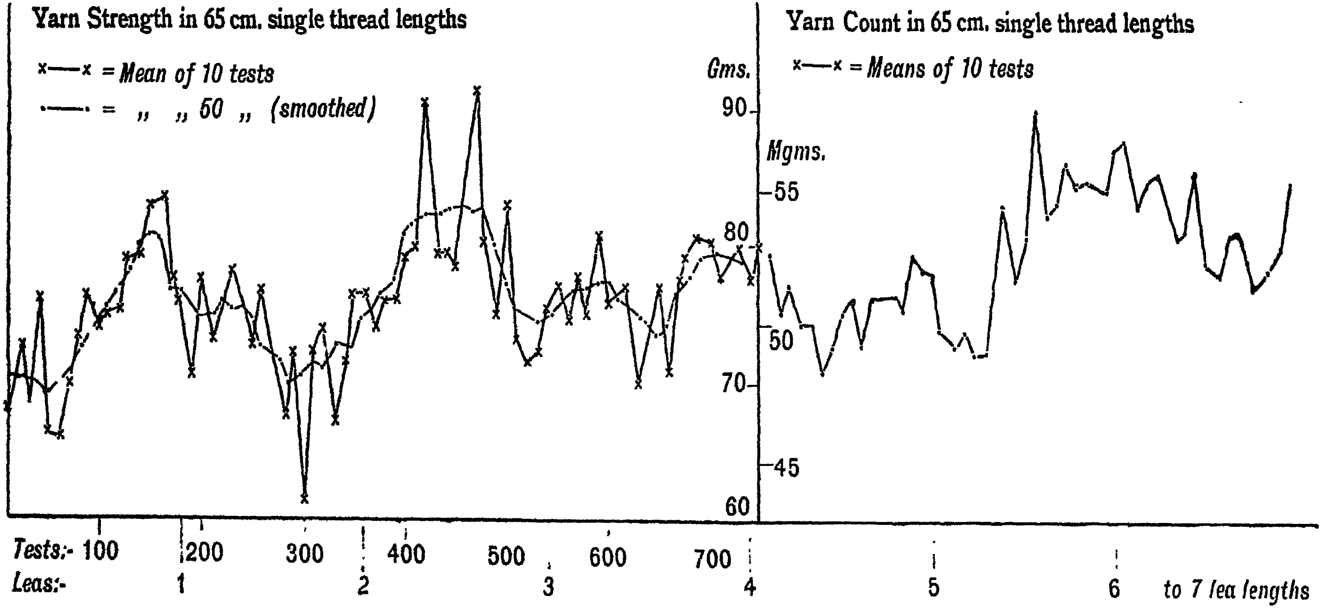

FIG. 31.—Sequential variation of a single strand of yarn in successive single-thread tests for "strength." Lengths of 65 cm. were tested consecutively for a quarter of a mile. Another furlong or so was then measured similarly for variations in the yarn weight, or count-reciprocal. Pseudo-periodic waves of various orders of magnitude are visible by either method.

The average of the ten weakest consecutive tests in the quarter-mile is 44 grams as against 60 grams for the average of the strongest ten. Measured on full-twist yarn of good quality, 96s count, spun from pure line No. 95.

The practical importance of such decipherment, as well as the theoretical interest, led me to devote a long time to the attempt to analyse these variations of diameter in ordinary yarn, but without success. Though there is reason to assume a fundamental identity in the drafting waves generated upon any given sample of cotton by all the machines, yet it is sufficiently distorted to make such a direct attack upon the problem hopeless. The mass of measurements needed was so great as to require the preliminary invention of a special calculating machine before they could be studied adequately for the detection of periodicities in the occurrence of these waves, no existing technique of computation being speedy enough. For a thousand measurements it was possible to employ the usual Periodogram calculation,[1] but in order to test out and compare all the Thirteen Samples we needed many thousands on every one of them. The limited sampling of one yarn which I was able to effect by the Periodogram had shown definitely that the yarn diameter varied periodically within the few feet of yarn thus examined. When my invention of the "Harp" (p. 358) made adequate sampling possible, the true state of affairs was seen to be much more complex.

Periodic variations certainly existed, but in excess. The length of the waves was so variable that some could be detected at all lengths between 1 mm. and half a metre in the same yarn; thus the mule waves and frame waves seemed hopelessly tangled together. Discrimination between these various lengths of waves with respect to their amplitude and their form, and the analysis of that form into harmonics, presented a colossal task, even though our rough calculation could be done at a hundred times the usual speed. Thus, while it was not at all impossible to push forward the accumulation of facts, it was evidently more reasonable to shelve the direct observations, and to return to experimental methods.

Meanwhile the most striking fact observed was that the periodicities displayed were heavily "damped." Two or three periods might be found coexistent in one part of a strand of yarn, while in an adjacent part it would be found that one or more of these would fade away, to reappear later, or to be replaced by a new period. The position of the problem is not now so

[1] Whittaker, E. T., *Edinburgh Math. Tracts*, 1915.

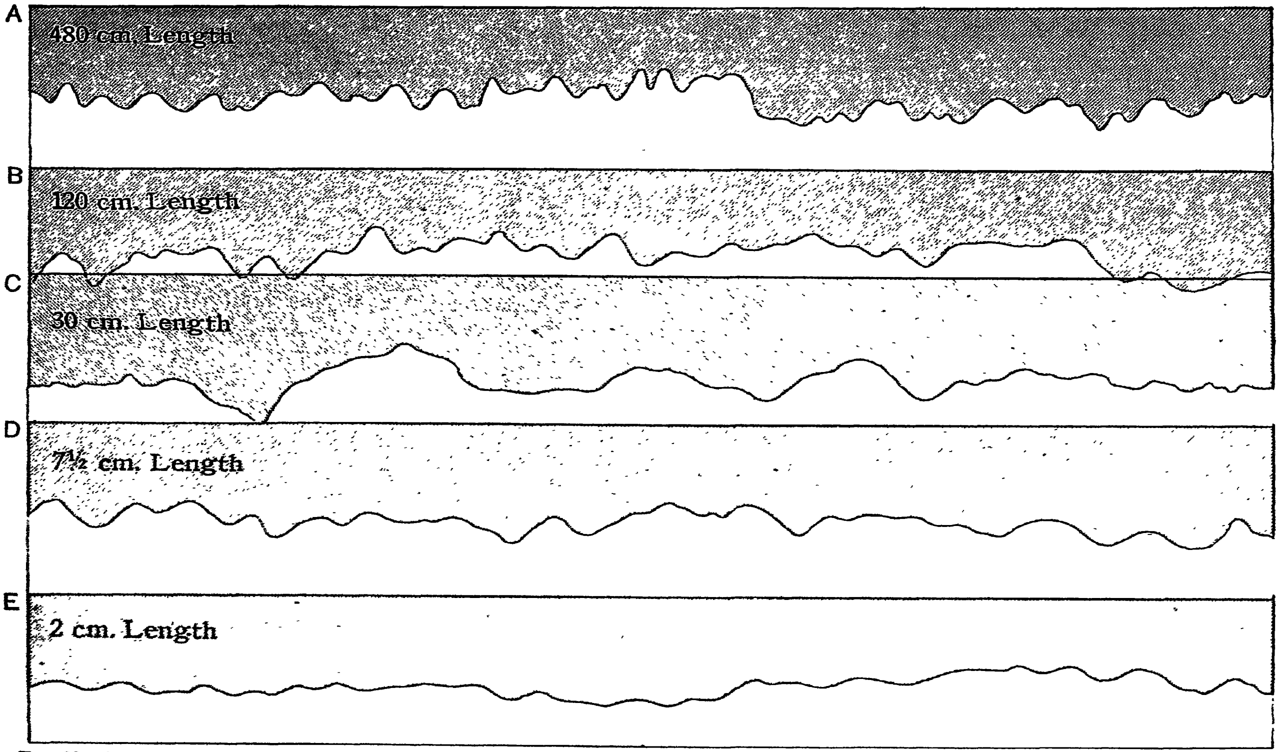

FIG. 32.—Sequential variation of yarn diameter, shown as Yarn Profiles. The five graphs show the profile of one and the same yarn in various places. In all five the scale of the diameter measurements is much greater than that for the lengths, the ratio being 4000 : 1 in A, and nearly 16 : 1 in E. While the diameter scale is the same in all five profiles, the length scale changes by fourfold steps, so that A represents some five yards of yarn, while E represents less than an inch. Waves of various orders of length can be seen from two metres downwards. C, D and E are traced direct from templets used in periodoscope measurements. A and B are redrawn from their templets to keep the same diameter scale as the other three. The yarn is a full twist from No. 22 of the Thirteen Samples.

hopelessly involved as it seemed to be when these observational studies were closed down, but I would admit quite frankly that although these observations showed the Drafting Waves to constitute a very real genus of phenomena, yet my knowledge of any specific drafting wave is too vague to merit discussion. When we come to consider their bearing upon the strength of yarn (p. 203) we shall be obliged to deal with their average expression instead of analysed details.

Nevertheless it can be clearly seen that no problem of textile technology can compare in practical importance, in theoretical interest, nor in unique peculiarities with this of the wave-forms, which are inevitably produced in the act of drafting by Wyatt's rollers.

Experimental Evidence

The experimental study of drafting happens to be reduced to very simple terms in the design of the Sorter (p. 361). This instrument operates by distributing a tuft of hairs which has been previously withdrawn from the sliver; the act of withdrawing this tuft is effectively the same as the taking of an instantaneous section through the ordinary time-sequence of the drafting process. The delivery rollers of the Sorter correspond to the front pair in a set of drafting rollers; the purpose of the Sorter makes it necessary that all the hairs in any one tuft shall be so gripped by these rollers that all their front ends are in alignment, and that they shall be withdrawn from the sliver without displacing any contiguous hairs whose turn to be gripped has not yet arrived. If any hair is thus displaced prematurely, it will be deposited on the collector in an erroneous position, which will ascribe to it a different length from that which it really possesses. Thus it is easy to measure the exact magnitude of this "Hair-displacement Error" by comparing the actual length of individual hairs with their nominal length as indicated by their position on the calibrated collector.

This Hair-displacement Error was the only serious obstacle to a satisfactory design of the Sorter, and although it was eventually reduced to insignificance, I never succeeded in abolishing it entirely. But if this unintentional displacement of a few hairs could not be entirely abolished, even by specialised drafting arrangements, it was evidently a very common event in ordinary

drafting, and several consequences follow from this, as will presently be seen.

I have already mentioned that the formation of drafting waves is commonly ascribed to imperfections which are no longer common in good drafting practice. My earliest experiments tested this by providing exaggerated defects in the rollers, such as a few steel rods fastened into odd flutes of the bottom front roller. Drafting nevertheless proceeded smoothly, the drafting waves remaining independent of these artificial irregularities. Helical and double helical flutes of varying pitch were also employed, to give conversely a smoother action than the usual parallel flutes, while by exaggerating the helix still further I obtained again a discontinuous nip which traversed the sliver from side to side. Smooth rollers of various materials, and grooved ones of various patterns, have also been compared. None of these modifications makes any fundamental change in the drafting waves when compared with the usual arrangement.

Attention was then turned from the withdrawal of the hairs to the method of their retention, and the methods of supporting the sliver were varied in order to reduce the Hair-displacement Error in parallel with work on the Sorter. The importance of using the sliver itself as the supporting matrix for the hairs was early seen, and from this there followed the logical conclusion that the sliver should be kept under control until the very nip itself of the front roller was reached. Later experiments showed that the relation between the accuracy of drafting and the length of unsupported sliver was so important that as much additional advantage appeared to be obtained by reducing the gap from four to two millimetres as by reducing it from an inch to half an inch. It has already been noted (p. 125), while considering current practice, that the use of rollers makes it impossible to reduce this gap to sufficiently small proportions, but the convenience of rollers is so great that I tried many modifications of their arrangement; such as a belt pressing on a large roller, or a grid of very small rollers through which the sliver took a serpentine path.

Presently it became clear that if the support of the sliver matrix were made adequately firm to prevent hair-displacement, it was no longer possible to draft the sliver in the ordinary way; the force required to withdraw a large proportion of the hairs—

a fifth to a twelfth of them in ordinary drafting—was quite prohibitive, demanding a firmer grip than the rollers could practicably obtain on the sliver. It was still feasible, nevertheless, to effect the drafting if this force were reduced by reducing the number of hairs to be shifted; in other words, by increasing the draft. Thus it became apparent that one way of improving drafting could be found, strangely enough, by making it more difficult; drafts of 200 were actually easier to effect than the ordinary high drafts. Such improvements as those of Casablanca and Jenninck are already known as "high-draft systems," because they enable drafts of 30 to be used, with the consequent elimination of a fly-frame from the preparation machinery. Consequently it is perhaps advisable to describe drafts of the order of 200 as "super-drafts," to avoid confusion. Such super-drafts eliminate not one fly-frame or two, but all of them; the sliver of the finishing draw-frame is pulled down into yarn at a single operation.

When the speed of the delivery rollers exceeds that of the feed rollers by such amounts as are necessary in these super-drafts, the feed of the sliver itself must be under micrometric control, moving forward hair by hair. So far as the mechanism is concerned there is no insuperable difficulty in providing this, but to ensure that this mechanical drive shall be transmitted to the front end of the sliver, so as to feed the front end of each hair into the delivery nip in its due and proper turn, is much less easy; the sliver is a soft and springy substance. It seemed to me initially that success must be impossible unless rollers were discarded altogether for reciprocating nippers, or the like, because certain considerations which are not unimportant in ordinary drafting become very serious in super-drafts. It will illustrate some of the weaknesses of ordinary drafting, which can be watched fairly well on kinematographic photographs, if some of these are mentioned.

Thus, there is no possibility of a regular hair-by-hair feed unless each hair is straight, and lies at right angles to the nip of the front roller. This at once eliminates the employment of roving twists; any hair which is anchored on one side of the sliver but is withdrawn on the other side will disturb the arrangement of its neighbours. It is also imperative to keep a "straight line nip," so that the axis of the sliver is tangential to all the rollers; such devices as the serpentine sliver path which I used

in the Sledge Sorter are thereby excluded, because a hair under tension can press down into the body of the sliver, thus altering the effective length of the sliver and causing its front end to over-run its true position. The ideal arrangement of the sliver matrix is therefore such as would be provided by a finishing draw-frame sliver compressed between two flat plates into a thin layer, with the front edge of these plates inserted as far as possible into the nip of the delivery rollers. The thinner the layer, the less the relative number of hairs which will lie on the margins, where the lack of support on one side makes conditions different from those obtaining in the rest of the sliver.

One of my colleagues [1] tried the plate method of compacting the sliver, in ignorance of its use for ordinary drafts in Jenny's patent. We found to my surprise that a steady feed of sliver was actually obtainable between two such plates when super-drafts were used, the top plate being moderately weighted. The reason for this became apparent when we realised that the force required to withdraw the hairs in front was sufficient to keep the sliver stretched taut between the front and back pairs of rollers; meanwhile the compression of the plates prevented it from springing back much during momentary easements of this tension. Thus the back rollers served merely to release the sliver at the determined speed, and not to push it forward.

This experimental draw-box was built with half-inch rollers and provided with a spindle for making its drafted sliver into yarn. Thus arranged on the laboratory bench, and fed with draw-frame sliver, it not only worked but worked well, producing several hanks of yarn with reasonable steadiness at ordinary average mule speeds of production. The strength of the yarn came within 10% of the normal mill strength for the cotton employed, in a count of 100s, which was produced by a draft of 200 direct from the finishing draw-frame sliver. The hairs from such a sliver emerge over a front too wide to let them be spun in the ordinary way, so that a pair of vertical collecting rollers had to be placed in front of the delivery rollers, simply to sweep the fringe of hairs together to one point, so that the twist could gather them in. The variation of count over long lengths disappeared entirely, in consequence of the uniformity of the much-laminated draw-frame sliver, but short drafting waves

[1] Mr. F. Hutchins.

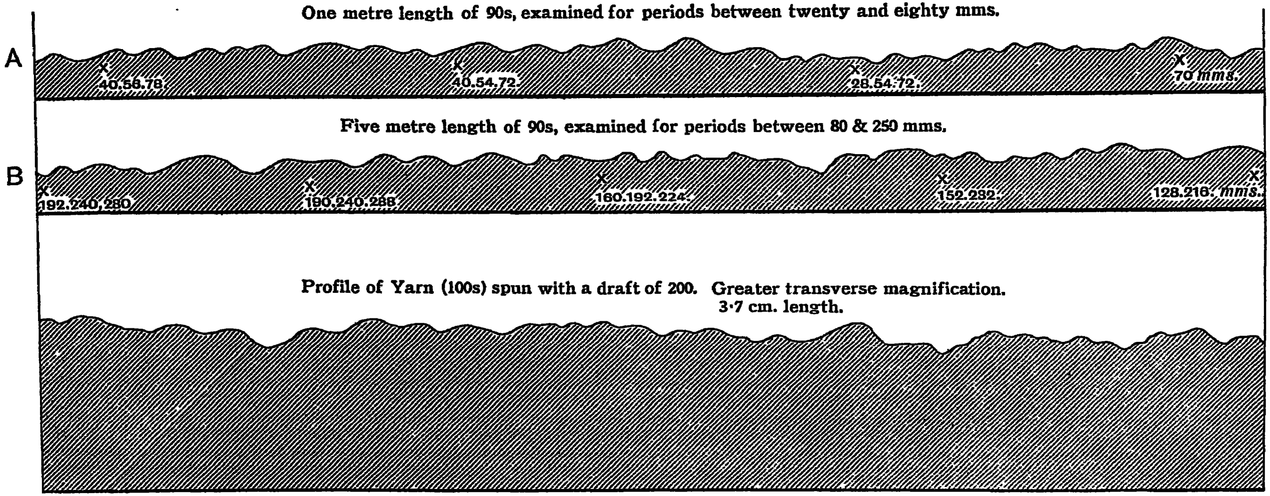

FIG. 33.—Examples of the way in which periodicities occur in portions of yarn-profile templets. Only one-twentieth part of the total length examined in each templet is here shown. The periodicities found around each centre are noted; they will be seen to be variable. In profile A the period (78, 72, 72, 70) occurs in all four searches, but (40, 40, 38) fades out to the right-hand end, as also does (56, 54, 54). In profile B, from right to left, the periods found at the five places searched may be shown thus:

.	.	.	.	128
.	.	160	152	.
192	190	192	.	.
.	.	.	.	216
.	.	224	.	.
.	.	.	232	.
240	240	.	.	.
280	288	.	.	.

were still generated; these latter were only a few centimetres long. Thus it was shown that while the plate feed was good enough to enable unprecedented drafts to be employed, thus enabling the great uniformity of finishing-draw-frame sliver to be utilised, yet it was not competent to keep perfect control of the hairs and so to eliminate the drafting wave entirely.

The persistence of simpler drafting waves in this super-draft yarn was no doubt in part due to imperfect packing of the sliver under the light pressure of the self-weighted plate; also to the longitudinal elasticity of the cotton hairs, which would still be exhibited in a sliver, even if it were packed perfectly solid. To eliminate the alternation of over-run with deficiency, which this latter cause would produce, we should have to reduce the gap between feed and delivery to really small dimensions. In the arrangement here described this gap was nominally that intervening between the front edge of the plates and the nip of the delivery rollers; actually it was much greater, because the definite grip of the feed upon the sliver matrix was situated behind the plates; thus the longitudinal elasticity of the hairs could come into play over a distance of several millimetres, or even over the whole length of a hair. Means for securing this really short gap are available to the hand-spinner by the use of finger and thumb.

Now it happens that such comparatively simple waves as were shown by this super-drafted yarn can be produced easily with the Sledge Sorter, though the serpentine sliver-path employed in this instrument allows the waves to be more emphatic than would be the case with a plate feed. To demonstrate them the Sorter is operated in the usual way, but instead of being rolled along the collecting plush after each tuft of hairs has been gripped, the instrument is held on the plush while its driving wheels are rotated by hand; each tuft withdrawn can thus be deposited in a separate place. On weighing a succession of such tufts individually it is found that their weight passes through a series of cyclic changes, rising and falling in a pseudo-periodic fashion. Again it is difficult to reduce this qualitative demonstration to exact measurement, but it is clear that we are examining instantaneous time-sections through the generation of drafting waves.

Following upon this demonstration it seemed likely that discontinuous drafting might be practicable, with the advantage of making time and space available for the introduction of sliver-

controlling devices. This was easily tested by adapting an ordinary Sorter to serve as a draw-frame. The delivery rollers were rotated continuously, delivering drafted sliver which a spindle compacted into yarn. The feed-box was worked intermittently, being pushed up to the delivery roller nip at regular intervals, which corresponded to each half-inch of yarn produced. Thus this yarn was built up from a succession of overlapping piecings, each piece representing one tuft of hairs. Far from showing any wave at half-inch intervals, these piecings were almost invisible, but the usual drafting waves ran on in just the same way as usual, disregarding the piecings entirely. This experiment finally proved that the drafting wave was entirely independent of elementary mechanical causes, so that it must be ascribed to the cotton itself.

Various experimental devices were tried afterwards, tending to return more and more to the action of the hand-spinner. The simple feed through the plates was modified by using much more pressure on the plate, but relieving the pressure intermittently, in conjunction with an intermittent advance of the feed.[1] Such designs have still to be fully tried out experimentally, but it is evidently very doubtful whether any complete solution can be reached until the actual controlling grip of the sliver is carried within a millimetre of the delivery nip. The only simple arrangement which would permit of this is obtained by substituting "doctor knives" for one roller of each pair; I have previously mentioned this as a fantastic suggestion (p. 125); it should now be evident that it is not merely fantastic.

We have quickly traversed the ground covered by a variety of preliminary experiments, and their general significance is probably clear enough for the purpose of this book. It must be emphasised, however, that they are all qualitative experiments; they are sufficient to show the trend of the facts, insufficient to measure those facts exactly. Progress in the study of drafting will continue to be slow until the construction of the drafting wave is understood analytically, so that the effects of such experiments can be expressed in terms of lengths and amplitudes of the waves. There are also possibilities of useful experiment in detailed study of the performance of individual hairs during the actual drafting; one apparatus which has not yet been

[1] W. L. Balls and F. Hutchins, Eng. Pat., 217,299, 1923.

worked was so constructed as to measure the momentary variations in pull between two pairs of rollers, while simultaneously collecting each hair for subsequent examination.

In any case, the reader should now be in a position to follow the broad qualitative implications of the observations and experiments, as also to appreciate the way in which drafting can exercise a quantitative influence on the structure and strength of yarn. Nevertheless, in order to evaluate the variation of the influence from one sample of cotton to another, we shall only be able to measure the final result produced by drafting variations upon yarn irregularity (p. 203); the intermediate stages are as yet obscure.

Qualitative Features of Drafting.—It will no doubt have been noticed by the reader that in the course of this chapter the point of view has been slowly shifted from the older one—which regarded the sliver as a plastic and homogeneous substance, moving like a viscous fluid—towards my view of the sliver as a particulate structure, with hairs for particles. Remembering that we already know these particles to be very various, so that the sliver is not even homogeneous, we may with advantage consider their unusual shape.

The manipulation of the particles in the act of drafting may be described as an attempt to slide any one of them into a predetermined position, without disturbing the position of any of its neighbours. Their shape is by no means well adapted for such treatment. As viewed along the axis of the sliver, they approximate to points; in side view they approximate to lines, and we have already noted that the length of the "line" is about two thousand times the diameter of the "point." The study of the relative movements of such elongated particles is somewhat peculiar to the textile industries; on the analogy of the word "aerodynamics" I have elsewhere suggested [1] that this study is sufficiently peculiar to be entitled "trichodynamics." At any rate, it raises some very curious problems. Indeed, the whole of textile technology might be described as an approximation to the use of one-dimensional units to build up two-dimensional fabrics, which are to be used for enveloping three-dimensional bodies, such as ourselves.

These hair units offer very little head-resistance to displace-

[1] *Nature*, 1921.

ment, so long as they are perfectly straight and are aligned along the exact direction of that displacement. In proportion to the deviation from such rectilinear form and movement, so the lateral resistance rises exceedingly quickly, because the friction of the immense lateral surface of the hair becomes increasingly implicated. The friction may become such that the particular hair unit is unable to slip any further; or if it continue to move it may carry other hairs with it, thus forming a clot. But since the hairs in this clot are dragged forward prematurely, an empty space must follow it. When the feed rollers bring this empty space along, the delivery rollers have no material given them to be delivered; at least, they will suffer a deficit of material. Thus the clot is followed by a thin place. But if the drafting process in any way depends upon the hairs to drag one another forward —as in ordinary drafting—this thin place will not bring its due quota of such hairs; these latter will accumulate until another thick place is produced. So the original disturbance will tend to die out, not in one single oscillation of thick place with thin, but in a series of such oscillations, gradually dying out, or being "damped" (p. 133). Mere consideration of the form of the hair units thus indicates that such a succession of thick and thin places, or drafting waves, is an inevitable consequence of the form of the hair units when passing through Wyatt's rollers, and in any drafting system where the movement of each individual hair is not definitely regulated.

Considering this mutual interference of hair with hair still further, we see that the Hair-displacement Error which can so easily be observed in the Sorter is merely the extreme case of a phenomenon which must be operative at all moments upon every hair during the act of drafting, so long as the hairs can touch one another while they are moving relatively to one another. Thus no amount of care in the preparation of the sliver can eliminate the drafting wave entirely; even if an ideal sliver were prepared, it would merely avoid accidental disturbances, but the fundamental wave-making phenomenon would continue; even with complete control of every hair in such a sliver it would still exist, though damped down to vibrations of microscopic length and amplitude. This is, of course, no argument for not making the best sliver possible; quite the reverse.

Eliminating the fortuitous disturbances, we can obtain a

rough analogy with the ideal sliver whose properties underlie any accidental happenings, if we play by hand with a heap of rigid steel sewing needles. If these are magnetised, so that they cling together, similar clots will form when we try to draft them. The factors controlling this tendency to clot, and so to form a drafting wave, are fairly evident in this analogy. The cause may be described as the "Slipperiness" of the needle, or hair; this slipperiness is itself a compound, built up of the coefficient of friction between hair-surface and hair-surface, and of an unknown amount of mechanical interlocking of the convoluted surfaces, together with the total effective amount of that surface on the average hair. The word "effective" is important, for it may well be the case that the friction is often between the crests of convolutions only. The hair-surface again is compound, depending almost directly upon the length, and also directly upon the "square of the (imaginary) radius," which we have seen to be much the same as the weight of the hair (Chap. IV).

Remembering that we are dealing with fixed weights of cotton when working to any given count of yarn, it will be noticed that the hair-surface increases exponentially as the finer hairs are used; the total mechanical resistance to drafting also rises rapidly with fine cottons. The mechanical resistance of the individual hair would seem to be less, however, and it is not in contact with any more neighbours than in a coarse sample; therefore the chances of any particular distribution being attained, or, in other words, that a drafting wave of some particular length and amplitude will be formed, is a problem in the chance distribution of individual particles. Consequently, if the drafting wave is expressed in terms of numbers of hairs, the fine cotton should give the same frequency distribution as the coarsest; other things, such as hair slipperiness, being equal. But as there are many more hairs in any given count of yarn when the fine cotton is used, the same number-amplitude of the drafting wave will result in a smaller diameter-amplitude.

The effect of hair Fineness upon drafting is thus double-edged. It makes the individual hair more slippery for physical reasons, while it decreases the amplitude of the drafting wave for statistical reasons. The effect of hair Length upon drafting would seem to be necessarily much less important; increased length decreases the number of hairs in cross-section, besides

decreasing the "slipperiness" by increasing the surface of each hair. These changes would actually militate against good drafting, but there is more than compensation in the additional overlap of hair on hair. Much of the advantage attributed to length as such is in reality due to a non-essential association between length and fineness, with which the next chapter will deal more fully (p. 149).

The clot-forming tendency of cotton is thus mainly controlled by hair slipperiness; an arbitrary comparative method for measuring this has already been described, by measuring the breaking load of untwisted roving (p. 70). In addition, it was noted in the preceding section of this chapter (p. 140) that the elasticity of the hair must also be implicated, so long as the gap between feed and delivery is of appreciable length; a springy length of sliver will be liable to over-run its assigned feed, springing back again afterwards to an extent which will be directly proportional to its free length.

When the drafting waves are eventually analysed, they may therefore be expected to show a very complex structure, such as my observations have already indicated. The waves should be expected to record purely fortuitous disturbances, due to imperfect preparation of the sliver leaving knots, neps, and crossed hairs therein; and also to consist fundamentally, beneath these, of periodic disturbances inherent in the kind of cotton employed.

The fortuitous disturbances may be exemplified by such extreme events as the presence of a tangled nep, or of a stiff jute fibre, in the sliver. The former would start a series of waves by dragging forward too many hairs, the latter by not bringing enough; such a disturbance would begin as a shapeless hump or hollow, which would set up a series of more definite waves behind it, these being rapidly damped and fading eventually into the underlying fundamental waves.

Though the fundamental wave forms may as yet be difficult to analyse, yet in my estimation there can be no doubt as to their real existence. We shall see later that when different samples of cotton are spun simultaneously, the distribution of weak places in them can be quite dissimilar (Chap. XIII) and that most of this distribution can only be ascribed to dissimilarities in the actual numbers of hairs left in the trough of the "average drafting wave." We have seen indications that the amplitude

of the wave may vary in different thicknesses of sliver, while its length remains constant (p. 130), which would now seem to be quite probable; if so, the length of the drafting wave would be a fundamental constant for a given sample. Undoubtedly this length will alter with the environmental conditions of the drafting, because it depends on the hair slipperiness, which will vary with the rigidity and the electrical charge of the hair, and with the condition of its waxy surface, which are all affected by the environmental humidity and temperature. Given a defined environment and one particular cotton, the amplitude of the wave might vary though its length was fixed. In this connection it should be noted that the mean amplitude of the wave across a wide comber lap is practically zero, because thick and thin places are formed locally at the same instant; not until thin slivers are reached does any disturbance take effect over the whole width of the sliver at once, as in the final draft at the spinning machine. It is for this reason that the Machines were discussed in an inverted order.

There is evidently no hope of attaining to perfect drafting unless the movements of each individual hair are precisely controlled. Conversely, there is every prospect of improvement in yarn quality if such control can be substituted for the chance distributions given by Wyatt's rollers. The spinner is already in possession of a sliver which is very near perfection, from the finishing draw-frame; it only remains to deal out from it each hair in its proper order, instead of setting up confused disturbances as at present. Whether this can be done by rollers, or whether the electrification trouble will have to be harnessed for the purpose of separating the hairs completely, only the future can decide. However it may be done, super-drafts will be necessary.

In spite of my present inability to analyse the drafting wave itself in quantitative terms, the utilitarian result of the studies outlined in this chapter is not altogether unsatisfactory. The primary function of drafting has been picked out from among its more assertive companions in the spinning mill, and its basal significance recognised. The principal responsibility for drafting defects has been traced to the cotton itself, putting the machines into their proper places as accessories. The possibility of spinning with drafts as big as 200-fold has been demonstrated, and the practical perfection of the finishing draw-frame sliver has thereby

been confirmed. The problems of drafting thus take upon themselves a very different appearance from that shown in the quotation from which we started on p. 81.

While it is sufficient for my present purpose in this book to show reasonable cause for finding—as we shall later find—that there are specific peculiarities from yarn to yarn with respect to the distribution of weak links, yet I hope this chapter may serve a secondary purpose by leading other students to undertake the quantitative study of an interesting industrial, experimental, and statistical problem.

CHAPTER IX

THE PRODUCT OF SORTED COTTON

It is not easy to weave one's material into an orderly arrangement when an intricate subject is being developed, as in these pages. Sometimes it is even advisable, after the material has been sorted, to introduce some of the threads prematurely in order to advise the reader that they will be more conspicuous at a later stage. Thus I have anticipated some of the material which otherwise would find its place in the present chapter, if this were an isolated essay about the influence of hair length on yarn properties.

Of such material is the grading of length (p. 10), and the analysis of length variation within a sample (p. 48); the relation of length to drafting (p. 144), and the Hair-displacement Error in its relation to the general purpose of the Sorter (p. 136); the good spinning of my short-staple pure line No. 95, and the poor spinning of No. 111 (p. 9), which had an exceptionally uniform length; the low efficiency of the comber (p. 110) as a length sorter also comes into the story, because its improving effect on yarn has been incorrectly attributed to such sorting. These are all relevant to the subject of this chapter; it is necessary to emphasise that relevance because the conclusions reached herein will be found heterodox, so that if they were to be read without reference to the general context, a misleading impression might be produced.

In outline the position is this:—All through the past history of cotton-spinning it has never been possible to make a direct comparison between two samples of cotton which differed solely in respect of their length alone. Always it has been the case that other differences have been implicated, usually such as could not be definitely evaluated. Several years after the Sorter was first invented it became possible to realise the aspiration which that invention had set, from the very beginning, before

Mr. McConnel and myself; samples of length-sorted cotton were prepared in bulk sufficient for their use in spinning tests. The results of those tests show that when samples thus differ in no other character than that of length, the advantages obtained by the long samples are not at all striking. Thus, while length-sorting has been shown to be practicable, its practical utility appears now to be very dubious.

But this result would seem at first sight to be contrary to common-sense, seeing that length provides the primary classification of cotton on the world's markets. It will be found less improbable when it is realised that there is commonly a non-essential association of length with other advantageous characters. If the general result is as correct as I have reason to think it to be, it is of some importance by indicating that other accepted opinions about cotton-spinning may also be true in particular cases, and yet not be absolute and unavoidable verities. More directly, the experiments recorded are of use in my thesis by throwing further light on the yarn-cotton relationship. To use them for this purpose it will be necessary to anticipate a later discussion of yarn strength when citing the strength of the yarn produced; this may perhaps be excused, because the yarns cited are built from hairs which are identical in every respect except their length.

The Usual Association of Length with other Characteristics.—I owe to a colleague [1] the idea of searching through our existing records in order to pick out five samples of cotton which most nearly resembled five experimental samples sorted out of one cotton, so far as their hair-length frequency-distribution was concerned. The five world's cottons thus selected provided a most striking example of the way in which length must unavoidably be associated, in everyday spinning, with other characters which bolster up its reputation.

The samples in question were a Burmese, a short-staple Upland, a long-staple Upland, a Sakel, and some commercial St. Vincent Sea Island. It will be noticed that these represent species of cotton, approximately *G. herbaceum*, *hirsutum*, *peruvianum*, and *barbadense*, and cover the whole range of the world's crops. When they were compared amongst themselves for characters other than length it was found that the ribbon width,

[1] Miss E. Lanigan.

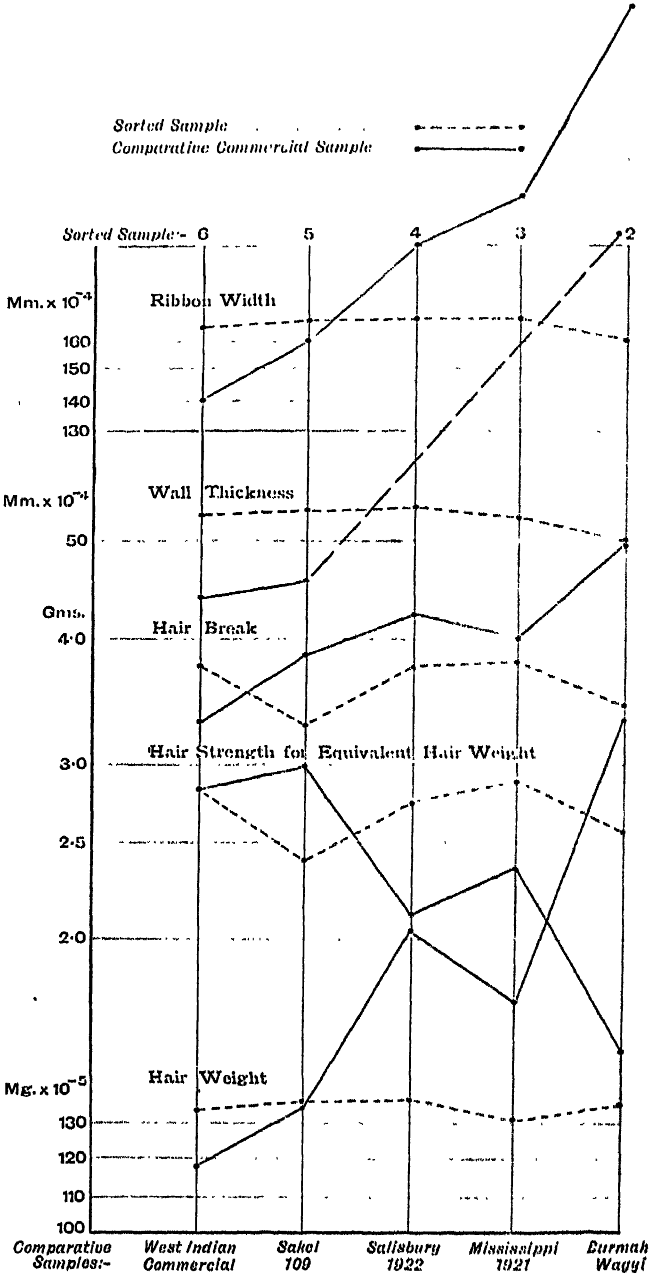

FIG. 34. See inscription on page 151.

wall thickness, and gross hair strength all decreased through the series of five as the hair length increased. Conversely, the hair strength per equal weight of hair increased together with the length, being roughly twice as strong in the St. Vincent as in the Burmese.

These five samples merely demonstrated the well-known fact that short cottons are usually coarser than long ones, though the demonstration was particularly clear. But it must now be emphasised that this connection is merely usual, not absolute. For instance, within the Egyptian group we may have a short cotton like No. 95 which is finer than a much longer cotton such as No. 77. Thus although there is a general tendency to correlation between length and fineness throughout the genus *Gossypium*, that correlation may be partly broken down in particular cases by the plant-breeder (Chap. XV).

A parallel case has already been noted in the variation of hair length on single seeds, producing sorter curves which are characteristic for each variety, although the sorter curve has a typical form for all kinds of cotton. So in the relation between length and fineness, the biology of growth seems to demand that, on the average of all kinds of cotton, the length of the cell shall only be developed at the expense of its sturdiness; though the reverse is true of individual seeds. Thus a mental association has grown up which is false in particular cases, though true in broad outline. Fine but short cottons have been discarded by the plant-breeder, though they may have been more valuable than accepted long ones.

To separate the length-characteristic from other hair properties

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FIG. 34.—The non-essential association of length with other characters of the hair.

Five samples were chosen from the cottons of the world, because their hair-length distributions most nearly resembled those distributions in the five samples fractionated from Sakel by the sorting machine.

The hair tests made on these five pairs of samples are plotted in this diagram, with the longest pair on the left, the shortest pair on the right. The tests on the sorted Sakel are shown as dotted lines and are practically constant through all five. The continuous lines show that the five world cottons have their differences in length associated with similar differences in all their other characters. As the length decreases there is an increase in ribbon width, wall thickness (and cross-section), hair weight, and hair break, while the "strength per equivalent weight," or Intrinsic Strength, falls off enormously.

Logarithmic ordinates are used, so that the slope of each curve expresses the percentage change of the character from sample to sample.

has never been possible in the past. Even separate pickings of different lengths, taken at different times from the same plants, would also differ in other respects, as I showed statistically in *Raw Cotton*. To draw a sharp boundary between the effects due to length, and those due to other characters, became possible only with the invention of the Sorter.

MACHINE-SORTED SAMPLES

It is unnecessary to devote any space here to description of the length-sorting device, beyond the references already made to the Sledge Sorter, and those in the preceding chapter. With the additional information available in the Appendix it will suffice to note that a larger machine was eventually constructed, and two sets of samples of five different lengths were sorted out from some Sakel cotton, seven years after the original invention, and three years after the workshops of the Experimental Department had been created.

The composition of these samples can be compared with those of the five world-cottons just described. The contrast is emphatic, because these sorted samples are identical in their average ribbon width, wall thickness, hair weight, and hair strength. They were also identical with one another in respect to their preparation history, but their comparison with control samples of combed and carded preparation which had not been sorted is less exact, since these had not undergone the additional disentanglement which the Sorter effects as an accessory to its primary function; internal evidence shows that the difference is not serious.

Comber laps of the original Sakel cotton were six-folded and re-drafted on the ribbon lap machine. The resulting lap was then fractionated on the Machine Sorter, which was set to collect six separate groups of staple length. No. 1 group consisted of neps and displaced hairs; Nos. 2 to 6 were those subjected to test. On re-sorting these five fractions with the Sledge Sorter they showed the following length characteristics in inches; the mean, or average, needs no explanation; the modal length may also be described as the most abundant length; the third figure is more or less similar to "staple."

Sample.	Mean length.	Modal length.	"Half-fall."
No. 1	—	—	—
No. 2	0·89	0·80	1"
No. 3	0·99	1·03	$1\frac{1}{4}$"
No. 4	1·06	1·18	$1\frac{3}{8}$"
No. 5	1·08	1·25	$1\frac{1}{2}$"
No. 6	1·17	1·38	$1\frac{3}{4}$"

Carded control. } Intermediate in length character between No. 5
Combed control. } and No. 6.

While the yarns made from these samples were being studied, the mechanical problem was allowed to lie fallow, but neither the study of these particular yarns nor the mechanical evolution of the sorting machine is as yet complete. I have, however, seen enough to have no doubt that length-sorting is a practicable addition to the resources of the spinning mill, if it should be found worth while. At the present stage of uncertainty in this latter respect it is most interesting to look back to the early days of this work, when Mr. McConnel and I were united in scepticism as to the commercial value of length-sorting, but were convinced of its practicability, whereas the general opinion was opposite in both respects.

Our present interest is in following the spinning tests of these five samples, bearing in mind that they are unique in the history of cotton-spinning, for although they ranged from staple of less than an inch to staple of an inch and three-quarters, there was no other difference between them. We have thus the first opportunity ever available for finding out what length of staple by itself does really mean to the cotton-spinner.

Spinning Tests of Length-sorted Cotton.—It will be realised that the comparison of samples which differ in length must present many difficulties. The count of yarn must be brought into the experiment, for a short cotton cannot be pushed up to such a fine count as a long one. The twist might also be implicated under some conditions, for the smaller surface of a short hair could be made to give the same total adhesion to its neighbours as that of a long hair if tighter twist were used for it. Fortunately this is not a serious experimental error if we work with full-twist yarns, near the optimum relation between twist and yarn strength; the data here given are confined to such yarns. The roller settings must be different for each sample, as well

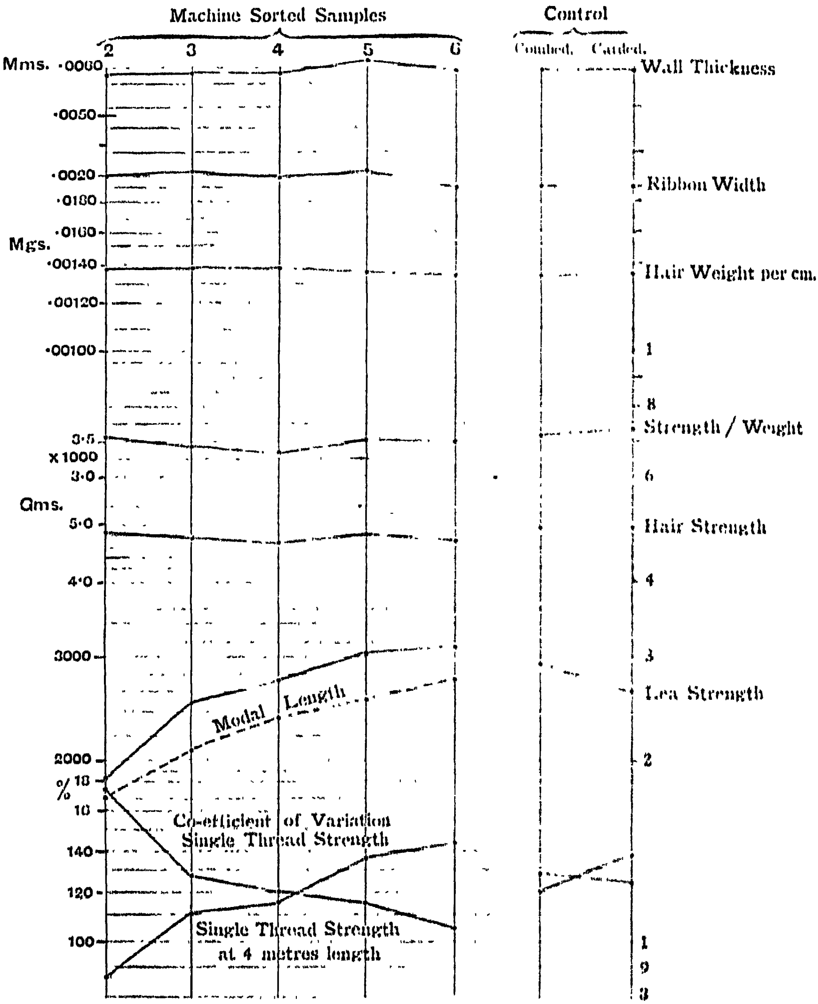

FIG. 35.—Tests for "strength" on the full twist 60s yarns made from five samples which differed *only* in their lengths of staple.

The modal hair length of each sample is shown as a dotted line. The curves for single-thread strength at four-metre lengths, and in the standard Lea test, are substantially parallel to this dotted line, which implies similar relative changes in all three, in consequence of the logarithmic plotting used.

The variability of the single-thread strength decreases as the hair length increases; actual distributions shown in Fig. 36.

as in each machine; an exhaustive search for optimum settings by using all likely variations of setting in each machine would involve thousands of spinnings and hundreds of thousands of yarn tests. Fortunately, as has already been shown (p. 128), the importance of correct settings increases rapidly as the roving becomes finer, so that this search by actual spinning of yarn may be confined to the roller settings of the spinning machine; the setting of the earlier machines is dealt with by trial runs and visual comparison of the slivers or rovings produced, until an optimum setting has been estimated.

The control samples of unsorted cotton were necessarily put through every roller setting employed. Thus the combed control went through the ring-frame at a setting of $\frac{11}{16}''$, as also at $\frac{21}{16}''$, and at all the intermediate one-sixteenth stages. When its behaviour came to be tabulated it was found that all these variations of setting made no difference in the strength of the 60s yarn produced, while in finer counts the effects were still below expectation. This is an interesting side-light on the importance of roller setting when working, as in this case, with a fairly light self-weighted top middle roller; it should be remembered, however, that in 60s we were spinning a fine cotton far below its upper limit.

Yarn Strength.—Many other points of similar technical interest were encountered in the course of these spinnings, but the purpose of this book will be served sufficiently by confining our attention to the strength of the 60s ring yarn, and attempting to analyse the cause of the strength differences there found to exist between the five sorted samples. Such differences cannot be caused by differences in hair strength, fineness, or surface properties, since all the samples are identical in these respects. This reduces the possible causes of difference either to the adhesion of hair to hair in the twisted yarn or to differences in the arrangement of the hairs during drafting. The former is not very probable, since we are confining our attention to full twists, and it will be shown that nearly all the differences found can be accounted for by differences in the occurrence of thin places. This chapter thus follows naturally upon the prior discussion of drafting.

It is necessary to beg the question of the meaning of statistical statements of yarn "strength" until Chap. XII is reached. For the moment it is sufficient to state that the following figures

are comparable amongst themselves, and that they show the relative strengths obtained when long lengths of yarn are tested, wherein the frequency of occurrence of weak places has ample opportunity to display its effects; the test is the standard lea test employed throughout the industry.

Sample.	Strength.	Ratio between strength and length.		
		(*a*) Mean.	(*b*) Modal.	(*c*) Half fall.
No. 2	65	137	123	154
No. 3	86	115	120	145
No. 4	93	114	126	148
No. 5	103	105	121	155
No. 6	110	106	121	159
Combed control	100			
Carded control	90			

On reading the three last columns vertically it will be seen that the relation between strength and length keeps nearest to constancy if the length is expressed as the modal value. The figures in the column for mean length rise steadily; those in the last column, which corresponds roughly to staple length, fall and rise again. It would therefore appear to be indicated—so far as a few figures can be trusted—that the important length of hair is the length which is most commonly found in the sample. This would seem to be a reasonable conclusion; if it is eventually confirmed, it has a practical bearing on the grading of cotton (Chap. I), where the modal length cannot be estimated by hand and eye. Here again it is true on the average of the world's cottons that the mode, mean, and staple lengths will be inter-related, but this relation must vary from sample to sample.

More important and more definite is the fact brought out by this table that the ratio of yarn strength to length of hair is practically constant; almost exactly so when the modal length is used. Expressing this in a different way—the lea strength of yarn in full twist is directly proportional to the modal length of the hair composing it.

But the limits of range of the world's cottons are approximately $\frac{1}{2}''$ and $1\frac{1}{2}''$ as regards their modal length; thus the maximum possible effect of hair length, *as such*, is restricted within narrow limits; the longest cottons grown will make yarn which is three

times as strong as the shortest cottons which it is worth while to cultivate. This conclusion, it must be repeated, applies only to the isolated effect of length, and only when both are spun well within their limit of count. Such a difference of three to one is trivial in comparison with the differences which variations in fineness can produce. Within the same group of American cottons it would be easy for comparatively small differences in fineness to swamp out the advantage which an extra one-eighth of staple length can give; this advantage, if proportional to the modal length, only amounts to some 9% in the yarn strength. It will be seen that this does actually happen when we come to discuss the Thirteen Samples in Chap. XIII.

The absolute strengths obtained in these spinnings were in themselves a testimonial to the skill with which my colleagues conducted these tests. The strength of No. 6 sample was by far the highest break for which I have any record from any source, whether experimental or in commercial practice. This was not entirely due to the sorting, for the combed control had a strength which was itself abnormally high, while that of the carded control only fell to a good normal value for combed preparation.

It is the general opinion of cotton-spinners that greater uniformity of length is advantageous, though it has been shown in a previous chapter that the exceptional uniformity of No. 111 pure line did not confer advantages upon it; my earlier book, *Raw Cotton*, made this its main thesis from the grower's side. It is difficult to test this opinion from the data here available. The modal length of the combed control sample should have given a strength of about 103 rather than 100, as compared with Nos. 5 and 6. This would indicate that the greater uniformity of length in the latter samples was actually advantageous, but it is offset by the fact already noted about the differences in preparation history between the combed control and the sorted samples. The point could be tested by deliberate re-mixing of sorted samples. Meanwhile it is clear that no very great advantage is obvious, and it is still possible that the early cotton-spinners were not in error when they mixed short cotton into long cotton, with the deliberate intention of improving the drafting; I am indebted to Mr. McConnel for information concerning this practice, which is now obsolete so far as the improvement of drafting is concerned, from the history of his own family.

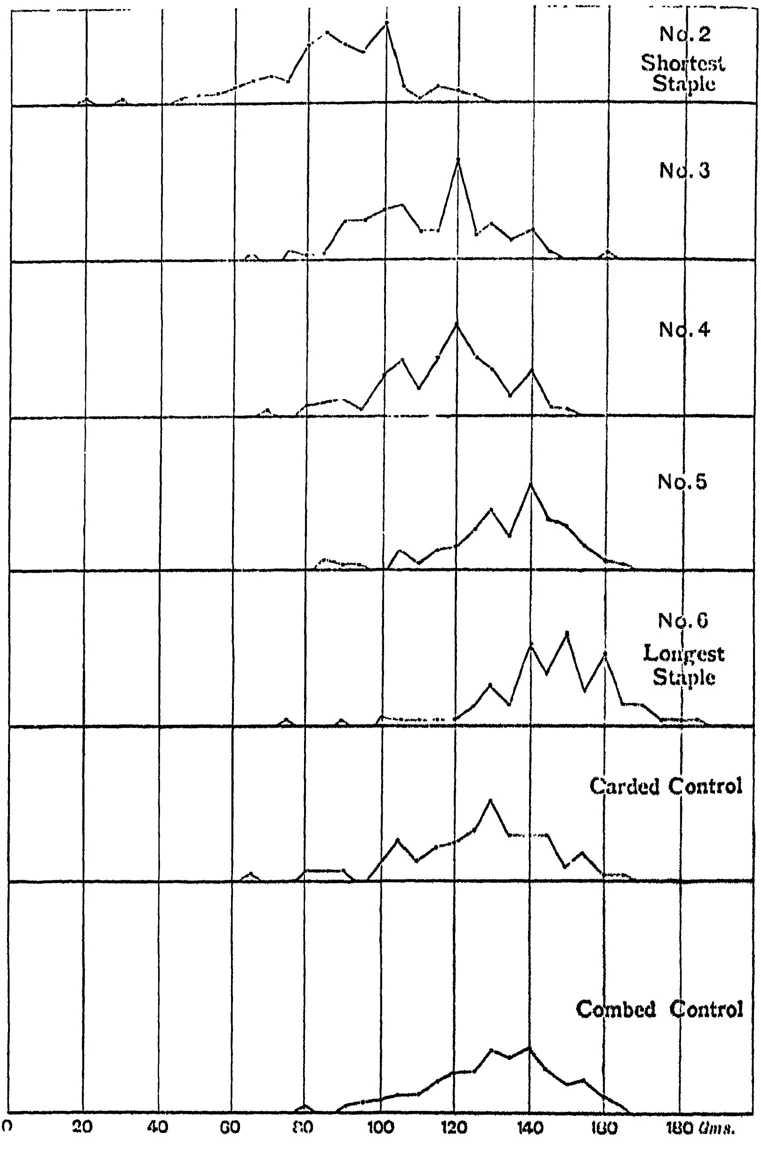

FIG. 36.—Single-thread strength frequency-distributions summarised in Fig. 35.

A lessened variability accompanies the increase in mean strength as the hair length increases. No. 6, the longest sorting, is definitely the superior of the combed control made from the original cotton without sorting.

Drafting.—Without anticipating the purpose of later chapters it will be permissible to call the attention of the reader to the fact that if these yarns could be tested in lengths less than the hair length, they would all be of the same average strength because the only point of difference between the samples would thus be eliminated. Although the average strengths would thus be alike, it might well be the case that the variability of the breaks would be much greater in the short samples, and this greater variability would provide more weak links, and so would lower the strength which we have found when testing the yarn in long lengths. Such variability would result from differences in the drafting, if it were smoother in the long samples but more and more wavy in the shorter ones.

But we are complicating matters by dealing with drafting in terms of strength, and can more easily visualise it by observations on the yarn diameter. Before doing so we may note that the frequency-distribution of single-thread breaks shows the mode to shift back as shorter samples are spun; the mode is accompanied by the maximum and minimum also, but not proportionately, for the short samples give more variable yarn than the long, the standard deviation being twice as big for the shortest sample as that for the longest. Similarly, when the yarn diameter was measured repeatedly, it was found that its variability increased as the hair length decreased; the mean diameter was, of course, the same in all samples.

It is convenient for the moment to express this variability of yarn diameter in the following way. Attention is confined to the 10% of places which were thinnest in each yarn; these are found to have an average diameter amongst themselves which is larger in the long samples, smaller in the short ones; the ratio of this diameter difference among these thin places is 13 : 10 as between the longest and the shortest. This does not sound very great, and it incidentally illustrates the difficulty of judging yarn by eye, since the eye can only estimate diameters. For it must be remembered that this difference in the diameter ratio must be squared, becoming 169 : 100, in order to indicate the difference in cross-sectional area, or in the number of hairs which are present in the cross-section of these thin places. The average number of hairs in cross-section can be obtained from the hair weight, and was found to be 70. The variability of

number of hairs found in these thin places can be tabulated thus:

Sample.	Number of hairs in average 10% thin places.
No. 2	28
No. 3	33
No. 4	37
No. 5	40
No. 6	43

If we now assume that a "weakest link" interpretation holds good for these well-spun, full twist yarns, and match these average numbers of hairs in the 10% of thinnest places against the strength, we find that the load carried by each hair in these thin places is constant, except in the shortest sample. In this shortest one the relative strength of a hair, as inferred from the yarn strength, is less than the constant; as this is absurd, it implies that the hairs are too short for the twist employed, so that they slip apart just before the full strength of the hair is reached. Otherwise we have the interesting and important result that for the remaining samples the strength when tested in long lengths of yarn is directly proportional to the occurrence of thin places as found by measuring the yarn diameter.

Sample.	Number of hairs in average 10% thin places.	Yarn strength.	Load per hair.
No. 2	28	65	232
No. 3	33	86	260
No. 4	37	93	251
No. 5	40	103	257
No. 6	43	110	256

More exact methods of thus analysing out the influence of drafting upon yarn strength will be discussed in later chapters, but this direct examination will have to serve our present purpose, for time has not permitted the full investigation of these sorted yarns. With all allowance for the preliminary nature of the experiments, it seems indubitable that the difference in yarn strength between Nos. 3 to 6 is simply and completely explained in terms of the variability of number of hairs.

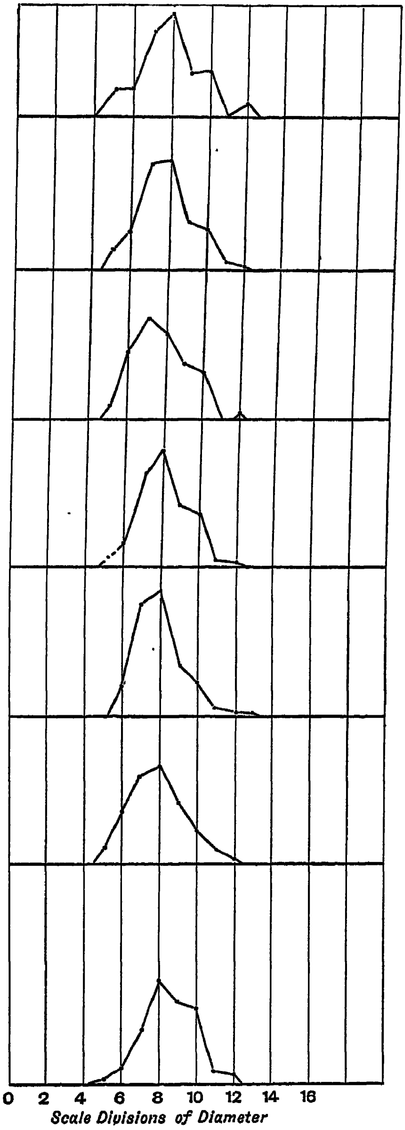

FIG. 37.—Yarn diameter frequency-distributions given by the yarns described in Fig. 35. Only a hundred sampling measurements are shown for each yarn, but the decreased variability with increase in hair length is evident.

This result may also be expressed by saying that the effective amplitude of the drafting waves is less in the long-haired sample. But this carries a further inference, since we have seen in the previous chapter that drafting might be improved; to wit, that if our drafting process were under control, we could make as strong yarn from short staple as from long, provided that the twist were sufficient. This conclusion needs qualification, however, for it is unlikely that the short-staple yarn would be as durable, since the tendency of yarn to untwist is an important factor in its working strength (p. 184); the deleterious effect of such untwisting will come into play more quickly with short hairs.

Summarising, the strength of the yarn from these sorted samples is proportional to the hair length because the latter affects the variability of number in the drafting process, and the consequent degree of irregularity in yarn cross-section controls the strength of the yarn as shown in long lengths.

Limiting Fineness of Yarn.—Both as a side light on the inherent reasonableness of the simple figures already given, and as an illustration of the way in which such figures may be utilised, it is interesting to examine their relation to the fineness of yarn. The determination of the limit of fineness to which a sample can be spun is a very tedious process, and any means of calculating that limit, however approximately, must be useful.

For example, in this 60s ring twist yarn the sample No. 2 has an effective minimum number of hairs in cross-section, by my arbitrary estimation, of 28 only, as compared with its average number of 70. In passing, the enormous variability should be noted, from 28 to more than 100 at least, even without taking extreme values. If this same sample were to be spun finer, it would be made into rather finer jack roving, and this would increase the amplitude of the drafting wave still further; let it be assumed, however, for our present purpose that the variability would merely be the same as when 60s were spun. Then the effective minimum number of hairs would be reduced from its present assessment at 28 right down to zero, or no hairs at all, if the mean number of 70 hairs were reduced to 42 hairs. Thus, with a mean of 42 hairs, the act of drafting would frequently cut through the roving altogether, and prolonged spinning would be impossible with our present drafting arrangements. Applying

the same rough argument to the corresponding rough data for No. 6, with its effective minimum of 43 hairs, its limit would be reached at an average cross-section of (70 — 43 or) 27 hairs.

Now it follows that if 60s yarn contains 70 hairs in its average cross-section, yarns which contain 42 and 27 hairs in that same section will have counts of 100s and of 155s respectively. These would be the limiting counts for sorted samples No. 2 and No. 6, and while it is not pretended that the data or the reasoning are more than illustrative, nevertheless these limit counts are sufficiently reasonable for such cotton as the third-grade Sakel on which this unique experiment was made.

It should be evident that the study of sorted cottons, even when merely sorted for length, can be of great service in helping to unravel some of the perplexities presented by the strength of yarn. If such material could be further split up into sub-samples of uniform fineness, their service to the student of cotton-spinning would then be enormous.

Sorting for Fineness

Although any notes on this subject must be mere speculation, in contrast to the definite experimental evidence about length-sorting, yet its potentialities must not be overlooked. To provide a method whereby cotton samples could be split up into fractions according to hair weight is an ambition which I have pursued unsuccessfully for many years. Even before coming to study cotton-spinning at first hand I had been so impressed by its possibilities that I ventured to write an article on the subject.[1] During the intervening years I have watched intently for some clue to a possible method, but have failed to find one which looked in any way practicable.

The difficulties seem to be greater than those of length-sorting, but this may be mere illusion. I was once told that the length-sorter was so obvious that it should never have been allowed a patent; perhaps the fineness-sorter is just as obvious, if we could only see the obvious!

I have already indicated the possibility of future development in the fractionation function while dealing with the machines (p. 118), and it was pointed out, when describing the variability of the hair (p. 50), that the range of fineness upon single seeds

[1] *Science Progress*, July 1915.

is of the ten-to-one order. The potential utility of sorting for fineness is to be found in this great range. As regards the diameter of the hair cell, the range of the world's cottons is about the same as their range in length. But the important thing for the cotton-spinner is not the diameter but the cross-sectional area, or the weight per unit length, or the number of hairs in the cross-section of a given count; all three are approximately the same quantity in different forms of expression. Now this quantity does not depend directly on the diameter, but upon its square; thus, where we have split a sample into six classes by length, we should have similar relative differences from the same sample when split into thirty-six classes by fineness.

As the same increase in range applies to individual samples of cotton, or even to single seeds, it follows that even in the finest and most uniform cottons—such as Dr. Harland's V. 135 [1]—the range is still sufficient to make fineness-sorting well worth doing.

But this range is to be still further increased, for some of the physical properties of the hair which depend upon its fineness are not even proportional to the square of the diameter; its suppleness and resistance to twisting depend on the cube, and even on the fourth power, of the diameter. Thus the diameter range of three-to-one opens out in some respects beyond nine-to-one and becomes 27 : 1 or even 81 : 1. This is a wide field for exploration.

A qualitative analogy will serve to illustrate the advantage which might accrue from even imperfect fractionation, such as the removal of coarse hairs. Any spinner knows how the ends come down when a fibre of jute finds its way into the mixing, partly through disturbance of drafting, partly through nip-ratching. One of my colleagues [2] noted incidentally that some cottons contained odd hairs which had not collapsed as usual into convolutions, but were true cylinders, very convenient for the calculation of absolute values in hair properties. Such cylindrical hairs, far more rigid than collapsed ones, would play the same part as jute fibres, though to a less degree, and it is

[1] W. L. B., "The Weights and Measures of the Cotton Hair," *Emp. C.G. Review*, 1925.

[2] Mr. F. P. Slater.

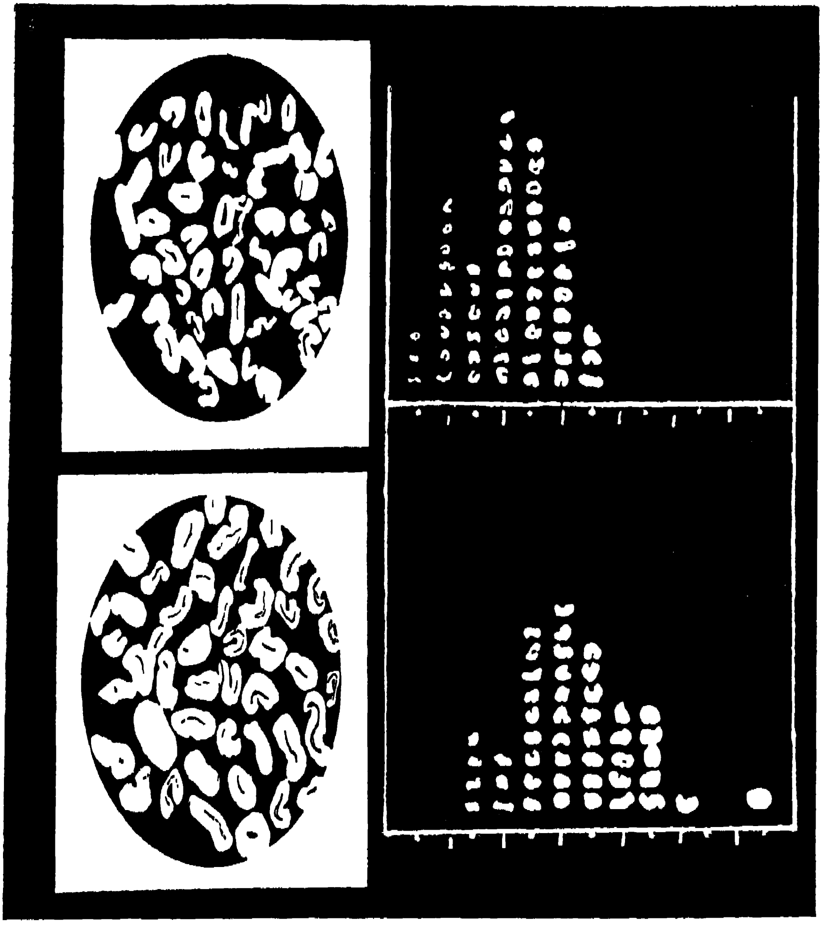

FIG. 38.—Illustrating the wide range of material which is available even in pure-line cotton, when attempting to fractionate for fineness.

Transverse sections of individual hairs taken at random from two samples of Harland's V. 135, the finest cotton known. One sample as grown by Dr. Harland in the West Indies, the lower one a trial culture in the Sudan by Mr. R. E. Massey.

On the left the sections appear at random as under the microscope. On the right they are grouped in classes according to their areas, as frequency distributions.

[*To face p.* 164.

significant that they have only been found in bad samples. But, to a less degree still, a normal coarse hair will have the same effect; the advantage of its removal is self-evident.

At the present stage we can do little more than guess at the differences which we should find between yarns spun from fineness-sorted samples. Greater suppleness would follow necessarily, and lower twist factors are probable, but the main influence would come through drafting. It is fairly safe to presume that, other things being equal, the amplitude of the drafting wave depends in some way on the number of hairs in cross-section of the roving; if we could double that number at any given count, we might obtain as good drafting at 600s as we now obtain at 300s; alternatively, which is more important, a better yarn could be produced at ordinary counts. Given the discovery of a method there is no difficulty in thus doubling the number; the range available would easily allow us to treble it.

In conclusion it should be noted by the reader that the industrial practicability of Fractionation will be settled by the super-value of the long and fine fractions. The coarse and short fractions will be worth less than the original raw cotton. Consequently the development of fractionation processes must go hand in hand with the specialisation of the industry, each reacting upon the other. An accurate analysis of the relationship between yarn and cotton must therefore precede both. Now although it would seem hopeless to expect that the super-value of the best hairs from crop lots of Sea Island should be able to pay for the depreciation of the residue which would ensue on their removal, yet one fact which has been passed under notice is very relevant to such costing:—though length and fineness are commonly associated in the various cottons of the world, as such (p. 151), yet it has also been found that this association breaks down within a single sample, partly or completely. Exact measurement of this association within a single boll has still to be made, in terms of single hairs, but that the association must at the best be very slight can be inferred from our knowledge of boll development.[1]

Thus it may become permissible to use the worst cottons, instead of the best, for fractionation. Odd lots of impure stock,

[1] W. L. B., *Raw Cotton*, Chap. IV.

crops of "samples," can be purchased at lower prices, which might pay the cost of fractionation. When thus split up into classes of uniform fineness and length, the residual heterogeneity due to the other hair characters of intrinsic strength and slipperiness would probably be less serious than the heterogeneity of fineness and length which is now found in the best cottons grown. This would make the spinner less dependent on the grower for the quality of his ordinary supplies, and for the steady run of such supplies from year to year. It would still leave co-operation necessary for achieving the best results (Part III).

CHAPTER X

THE ARRANGEMENT OF TWIST

THE two previous chapters have dealt, directly and indirectly, with the causes which ensure that every strand of singles yarn is built up as a succession of thick and thin places, very unlike the ideal cylindrical form. This lack of uniformity in the Number of hairs distributed by drafting provides variations in yarn Diameter which, in their turn, bring about variations in the Twist which is found from point to point along the yarn. This local variation of twist thus links up the present chapter to its predecessors, while the further examination of this local variation will be found later to provide an important clue for use in the analysis of yarn strength.

The distribution of twist can be conveniently examined under three headings: its local variations along the yarn, its average behaviour, and its artificial arrangement under the influence of the machines. It is advisable to set some limitations on the discussion before doing this.

The function of the twist is to bind the hairs together, thus bringing into play the coefficient of friction between their surfaces, and the total area of that surface. I have already mentioned (p. 80), as my "second paradox" of cotton spinning, that the behaviour then required from the hairs is the reverse of that demanded during drafting. This adhesion between the hairs, though effected by the twist, belongs to a category of phenomena that will be more conveniently taken into account in the next chapter, thus leaving the present one free for the more geometrical phenomena of twist as such.

Further, while these phenomena are essentially simple aspects of solid geometry, nevertheless no rigid treatment of them as such is as yet advisable. The yarn can only be regarded as a cylinder when infinitely short lengths are considered; the cotton is heterogeneous, so that very dissimilar hairs are found in every

cross-section of the yarn; the individual hairs are semi-plastic, semi-rigid, varying in their properties with changes of atmospheric environment, so that the yarn is never at rest, but must always be writhing, creeping, or straining as the hair dimensions and properties alter with each puff of air. Only a rough sketch is permissible.

An extension of this conception of instability to the changes of twist in yarn under load will be followed up later (Chap. XII); for the present chapter such instability need only be considered lightly.

Lastly, the discussion will be limited also by taking the actual shape of the twist for granted, as if the hairs were always arranged in the most stable solid pattern. That pattern would be simulated by twisting a cylindrical bundle of parallel copper wires, whose ends had first been soldered together to prevent relative displacement; under such conditions the central wire remains straight, and can be withdrawn afterwards if the twist be slightly eased; this may be described as "solid twist." But this is by no means the invariable pattern of hair arrangement after spinning. When quarter-inch lengths of yarn are watched during the act of untwisting them, as when using the Yarn Clinometer (p. 349), it is easily seen that they vary in the type of twist pattern displayed; there are two main types, which may alternate in successive inches of the same yarn, or even more frequently. One type is that already described; the other would be simulated if the same copper wires were laid out as a ribbon of parallel wires, one wire thick, before their ends were soldered together; if such a ribbon is wound round a central core (which may be real or imaginary) a pattern of twist is produced wherein every wire is of the same shape as its fellows. By simple experiments with such wires, or with strips of paper and rubber, it can be shown that the formation of one or the other type of spiral pattern is partly determined by the tension applied, and by the width of the ribbon; this "flat twist" is formed under low tensions, the "solid twist" at bigger tensions. In actual spinning the hairs emerge as a ribbon-formation, but the width of the ribbon varies, as does the tension also, and such alternations of twist pattern are thus quite probable. For simplicity's sake these changes will be disregarded, and the discussion based upon a "solid twist" pattern, merely remembering

that this pattern may vary round a statistical average in any one yarn, as also from one setting to another on the same machine.

The Detailed Distribution of Twist

Just as the uniformity of the finishing draw-frame sliver was the pivotal fact upon which the discussion of drafting turned, so the pivotal fact for discussion of twist is embodied in "The Spinner's Rule." This rule, by which the spinner is guided in his decision as to the amount of twist to use for any given count of yarn, asserts that the twist should be proportional to the square root of the count. The square root is multiplied by a "twist factor" to give the turns of twist per inch; this factor varies somewhat from one kind of cotton to another,[1] and within any one kind it is modified between limits according to the kind of yarn required, whether soft weft, full twist, or double-spun. The minimum twist factor I have reached on the mule with good running was 1·7, but with experimental modifications of the standard machine. Ring spinning has higher minima, though these have also been much reduced in the Rule frame (p. 103).

As the existence of the Spinner's Rule is very convenient, giving us an absolute mode of expression for twist in place of memorising the appropriate twists for different counts, it is worth closer examination before proceeding to examine its micro-distribution. The count of yarn is the reciprocal of its weight; the turns per inch are as the reciprocal of the length of a single turn. Thus the spinner's rule may be re-stated as "The length per turn is proportional to the square root of the weight per unit length." But the weight per unit length is proportional to the cross-sectional area; this square root of the weight is thus equivalent to the square root of the area, and this is simply equivalent to the radius or diameter of the yarn. So the rule now becomes, "The length per turn is directly proportional to the radius of the yarn."

But this re-statement of the spinner's rule makes it obvious that the whole question is one of angles. This is easily visualised by considering spiral lines drawn on paper which has been wrapped on the surface of cylinders; unwrapping the paper for convenience in examination, we see that lines drawn at the same

[1] Thornley, T., *Advanced Cotton Spinning*, London, 1923, p. 491, and *Self-Acting Mules*, Manchester, 1906, p. 495.

angle to the cylinder axis will make complete turns in lengths which are directly proportional to the radius of the cylinder on which the paper was wrapped. From the spinner's rule we

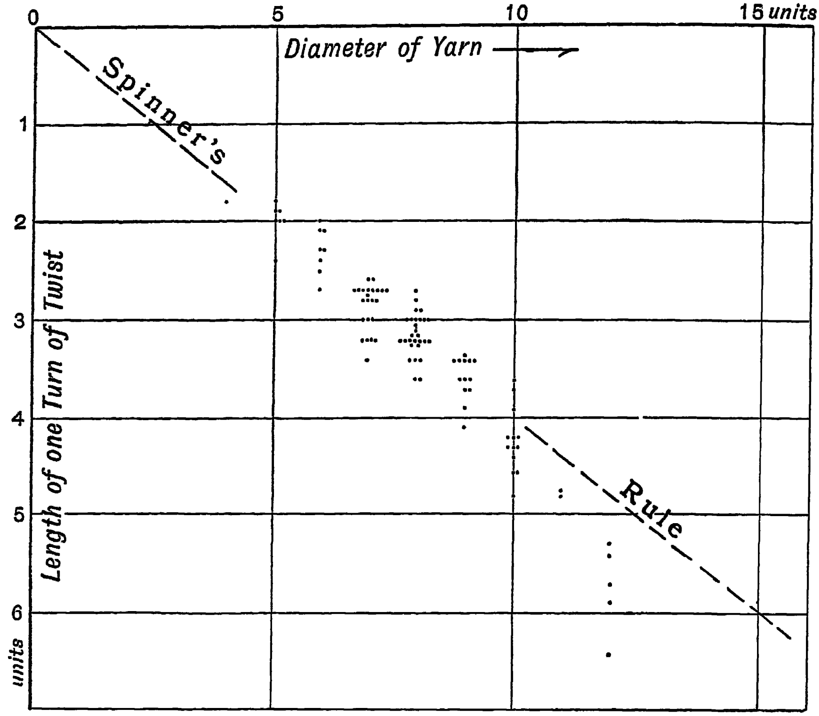

FIG. 39.—The Spinner's Rule governing the twist on a small scale.

Twist measured on 7 mm. lengths of a yarn, and the diameter measured concurrently in each length. Every such pair of observations is represented by one dot on this correlation diagram.

The particular yarn chosen for this presentation is the full-twist 60s produced by the shortest of the machine-sorted samples, No. 2 of Figs. 34–37, in order that the diagram may cover a wide range of diameters. The twist is represented as the "length of one turn," so that the relation with diameter which underlies the Spinner's Rule may appear as a straight-line grouping of the dots.

The deviation of the thick places from the Rule, in yarn thus measured without tension, is quite noticeable.

thus infer that the fundamental constant of twist is its Angle. It may here be noted that the range of twist angle is not great; it lies near 25° for fully twisted yarns, while 18° and 35° cover the softest and hardest yarns which it is usual to produce; small changes of angle thus make big differences to yarn properties.

It should be noted that the " angle " of the twist here quoted is that of the hairs which lie on the surface of a " solid twist " cylinder. Hairs which lie nearer the centre will have a smaller angle, till the zero angle is reached at the straight hair in the centre.

The spinner's rule is common knowledge, and its analysis into angular measurement is probably not novel, but its investigation on a small scale was first made practicable by my Yarn Clinometer (p. 349). Lengths of about a quarter-inch of yarn are examined in this microscope accessory, and the number of turns are plotted concurrently with the yarn diameter. Broadly speaking, the proportionality is complete, so that the spinner's rule does not merely hold good for the yarn as a whole, but is true for every part of it, however small. In other words, a spiral bundle of varying diameter so adjusts itself eventually under longitudinal stress that the angle of the surface spiral is invariable.

The minor exceptions to this Micro-spinner's Rule are, as is usual with exceptions, very informative. The principal one relates to the thick places, and though it can be illustrated from any set of data, it will be convenient to use those which have already been partly quoted for variable yarn made out of the short Machine-sorted Sample No. 2 (p. 152). If these are classified according to the radius of the yarn in millimetres, and the average length of a turn is ascertained for each class, also in millimetres, the first hundred observations on the Clinometer provide the following table:

Radius.	Length of Turn.	Ratio of Length/Radius.	Angle of Surface Twist.
0·043	0·550	12·8	26° 10′
0·052	0·630	12·1	27° 5′
0·060	0·790	13·1	25° 10′
0·069	0·870	12·6	26° 15′
0·078	0·940	12·0	27° 20′
0·086	1·190	13·8	24° 30′
0·095	1·330	14·0	24° 0′
0·103	1·580	15·4	22° 15′

Thus the angle of twist is sensibly constant until the three thickest groups are reached; in these thick groups it steadily decreases, so that they are under-twisted and would seem to be

more liable to break by slipping. Actually this need not be so, because these measurements are taken from yarn which has never been placed under big tensions for any length of time; a few grams in the ring-frame for a few seconds only. If tension be applied and maintained, its effect is to bring about further equalisation of the twist angle, since the tangential component into which a part of this tension is resolved by the spiral structure serves the purpose of driving twist into these thick places, against the leverage and the internal friction which their greater radius exerts. Thus a re-measurement of the twist angle on this same yarn after prolonged tension will show less deviation from the constant angle of the spinner's rule than when the measurements were made, as quoted above, on yarn lying free.

This process of Readjustment of Twist is of prime importance. It can happen while a yarn is being stretched in a breaking-load test, and does habitually so happen; *e.g.*, a hair may be prematurely broken by local concentration of stress upon it, the rest begin to slip apart, but the incipient break is arrested by rapid alteration of the twist to fit the reduction of radius. Thus, and especially in an irregular yarn, varying much in diameter, and built up of comparatively tangled hairs, the final break may be preceded by many incipient breaks, with continual twist-readjustment.

It is also important to note that the constancy of the surface twist shows the whole diameter of the yarn to be effective in resisting tension. If the strength of the yarn depended on a central core of hairs only, and not on the full cross-section, then the angle of surface twist would change progressively all through the table given above, since the twist angle shrinks to zero (p. 168) as the axis of the yarn is approached.

Examination of the spinner's rule under the microscope thus shows the yarn to be not only a self-adjusting structure, but one which employs all its constituent hairs to take their fair share of its burdens, so long as the yarn is freely suspended, and not prevented by contact with its neighbours from making these twist-adjustments.

Further, we have already seen some reason to doubt (p. 106) whether the conventional explanation of the action of the mule, that it draws out the thick places, will bear examination, except when such places are very thick indeed, like "slubs." It now

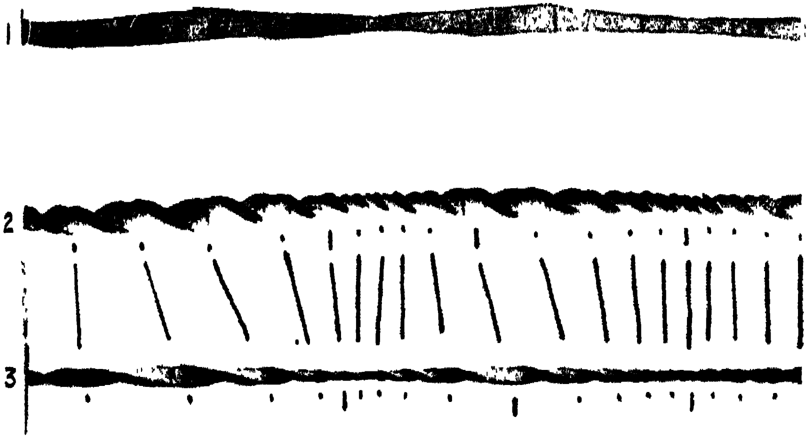

FIG. 40.—The simplest case of readjustment of twist, illustrated by stretching an irregular strip of rubber.

(1) Photographed unstretched and without twist; marked at regular intervals.

(2) Ten complete turns of twist put into the length. The constant twist-angle of the Spinner's Rule is apparent, so that the twist is shorter in the thin parts.

(3) The same twisted length was then stretched from the 20 cm. length of (2) up to a length of 30 cm. in (3), the latter photograph being taken from a greater distance, so as to restore it to the same apparent length as before. On comparing the dots and dashes which mark the half-turns of twist in (2) and (3) it will be seen that the stretching has caused further concentration of twist in the thin places.

In this simple case the latter change is merely due to the greater strain, or stretching, of the over-stressed thin places.

[*To face p.* 172.

seems more likely that the advantage of the mule in ordinary running, when the sliver is showing normal drafting waves, is not due to any such subsidiary drafting action, but simply to the fact that it provides some ten seconds of time and more, during which the stretched yarn can get nearer to complete readjustment of twist, as compared with the time available during a hasty passage through the ring-frame. It need hardly be added that readjustment is almost entirely prevented as soon as the yarn is wound upon the cop or bobbin.

It is now evident that the forces which determine the micro-distribution of twist in a yarn are such as necessitate the consideration of a Time Factor. If a thick roving be twisted up without tension, the distribution of twist will be determined by the radius of the roving, but the effect of the radius will be disproportionately great in the thick places, owing to the softness of such roving; the twist will therefore be excessive in the thinner places. As more tension is applied, so the radius of the thick spongy places will decrease proportionately more than that of the thin places, which are already more tightly twisted. More movement of hair on hair has thus to take place in the thick places, over and above the fact that there are more hairs to move. These movements take an appreciable time, whether in the obvious case of a thick roving, or in the less obvious case of yarn, and this applies even if the permanent deformation of the hair be left out of account. Indeed, when yarn is readjusting under a change of tension the movements can be seen and timed as bodily revolutions of the yarn, which may continue for many minutes when lengths of several metres are loaded.

Consequently it may be expected that the duration of exposure to the twisting force would have some effect on the exactitude with which the yarn follows the micro form of the spinner's rule. It is not easy to obtain exact comparisons between mule and ring-frame, or even between various durations of exposure on either of these machines, because other factors will also be varied. The rule-frame can give direct comparisons, by varying the free length of yarn between roller and spindle, but a more interesting equivalent case can be cited from the doubling of two-folds. The same spinner's rule applies with these as with singles, and the two-fold diameter and the two-fold twist angle are therefore closely correlated; it was found that the extent of this correlation

was markedly decreased when the duration of exposure to the insertion of twist under tension was halved. Yarn which is exposed for the minimum time will have more regularly distributed twist; that which is exposed for longer times will have its twist distributed irregularly, because that distribution will be more closely correlated with the distribution of thick and thin places. The former will be a less stable structure and therefore weaker, but its appearance—in the case of a two-fold—will be more symmetrical and pleasing. This short exposure can be obtained by high speed of production, or by decreasing the distance intervening between drafting rollers and spindle. The existence of a time factor in twist distribution thus helps in the partial antagonism between output and quality (p. 89).

The Average Twist

It is logical to describe this section as dealing with the average effects of twist, in contradistinction to the local variations just discussed, and to the artificial distributions effected by the machinery (p. 177). Nevertheless it will be well to remember that the "average" is probably not often a true statistical average of the local distribution. In so far as twist is employed for the structural purpose of binding the hairs together, it is probable that a few revolutions of the spindle—some thousands of millions daily—are wasted in slightly overtwisting the thin places to provide a kind of "factor of safety" for the thicker ones. The improvement of drafting (Chap. VIII) would therefore seem likely to result in an economy of twist; such economy would be most marked in the soft yarns.

Using the expression "average twist" in this loose sense it is not necessary to discuss it at any length. The study of twist is full of pitfalls for any quantitative treatment, because the structural effect of twist depends on the resolved radial component which acts to bind the hairs together when tensional stress is applied, and the adequacy of this radial component can at present be inferred only from observations on the breaking load of the yarn. Such observations not only involve the factor of safety already mentioned, but also the conditions of adhesion between the hairs—to be sketched in the next chapter—and the extent to which permanent set has taken place in the hairs themselves. Thus, if the effect of $2n$ turns in a unit length of yarn

has to be compared with that of n turns, it would be only reasonable to expect differences in the comparison if it were made between two such yarns when the cops were newly spun, or when they had been " conditioned " ; and again, if n turns were added subsequently to the spinning of the n-turns yarn, or if, conversely, they were subtracted from the $2n$ yarn.

Feeling my way through such considerations as these during the early stages of these studies, I concluded that the effective study of twist was only possible by deliberate spinning of yarns with various twists. Such experimental study was not then possible, and had to be made much later; such spinnings have since been made and published by other students also.[1] In the meanwhile I devised a Twist-Yield tester (p. 364) which gave sufficient information for my purpose, and found subsequently that this information differs less markedly than I had anticipated from the results obtained by deliberate spinning. The test gives data from which a curve can be drawn to show how the strength of yarn already spun is increased or decreased when its twist is altered. It will be seen from the Thirteen Samples (Chap. XIII) that the curve differs from one sample to another, and that it has a very characteristic form. The removal of turns from the full-twist yarn causes a rapid fall in strength, the curve plunging steeply to zero in the neighbourhood of 1·5 twist factor. There can, of course, be no doubt that if the curve were investigated in the opposite direction, by adding twist to roving, the change would not be quite so sudden in this region, because the hairs would not be set into helical form, as they are when the yarn is untwisted (p. 193). Conversely, as twist is added the strength tends to rise slightly further to a maximum, and then falls slowly until, at very high twists, the yarn will " snarl " and lift a weight which cannot break it.

This test is capable of very useful development. The objection due to the spiral set of the hairs can be largely overcome by starting with several different initial twists, and working round cycles equivalent to hysteresis loops. Thus, besides simply untwisting a weft yarn, it can be twisted up to full twist, and then untwisted from that point in comparison with a spun full twist. The differences between " weft cottons " and " twist cottons " can be investigated in this way, and the measurements

[1] *Vide e.g.* Oxley, A. E., *Jour. Text. Inst.*, 1921.

can be linked up with those upon the rigidity of the yarn in torsion and extension.

As this book is primarily concerned with the strength of yarn, the only twist which interests us directly in singles yarn is the optimum, or full twist. This may be described as being the minimum twist angle which will resolve a sufficient fraction of the tension into pressure between the hairs, so preventing slip under the given conditions of hair rigidity or surface, and allowing the full tensile strength of minimum cross-section to be utilised. This fraction ranges between 4% and 10% of the tension, according to the twist angles used. The various twist factors are, of course, well established in current practice, since spindle revolutions are too costly to be wasted; thus may be quoted [1] such values as 4·00 for ring twist from Indian or American cotton, 3·75 for the same cotton in the mule, and 3·61 for Sakel mule twist. Now the fraction of the tension which is available to bind the hairs together is related to the tangent of the twist angle; as this angle decreases from around 25° in full twist to 18° in super-soft experimental spinnings, so this fraction decreases to such an extent that more than twice the tension would be required to resolve enough lateral pressure. Thus the resolved pressure falls rapidly as the twist angle decreases until it is insufficient to balance the tension, and the strength of the yarn drops to that of sliver, long before all the twist is removed.

Our principal concern is to notice that a fairly flat-topped optimum exists in the twist-yield curve, where the rapid rise of the resolved pressure with increase of twist comes into intersection with a fall in strength due to over-twisting of the thin places. This latter develops when that component of twist which acts in tension along the axes of the hairs themselves begins to approach hair-break values in localised stresses. This optimum can be raised higher in singles yarn by double-spinning, wherein additional twist is put in while the original yarn is wet, and the pre-existing local stresses have thus been eased off by the softening of the hairs and by the lubrication which the water provides.

[1] Thornley, T., *Cotton Waste*, Scott, Greenwood, London, 1921, p. 361.

ARTIFICIAL DISTRIBUTION OF TWIST BY THE MACHINES

It has been shown (p. 171) that the local distribution of twist is dominated by the variations of thickness caused by the drafting waves, under the control of the spinner's rule. Strictly regular distribution of the twist by the machines cannot, therefore, be obtained, though some compromise is effected if the time factor is small. The nearest approach to such regularity can be obtained in a fly-frame when the nip of the drafting rollers is brought as close as possible to the nose of the flyer.

There are, nevertheless, some points of interest remaining in the artificial distribution which the machines impose upon the general distribution of twist in fairly long lengths of yarn, whether permanently or temporarily.

Insertion of Twist by the Mule Spindle.—This is a very temporary distribution indeed. It was conveniently examined by a colleague with the stroboscopic vision which the neon lamp provides, in conjunction with a minor device of my own. The interest of the observations is concentrated in the actual slip of the coil over the tip of the spindle, the process of winding up another coil to replace it being quite straightforward. Under ordinary spinning conditions the duration of the slip, from start to finish, is about 0·0003 second only, and it is not exactly synchronised with the same phase in every revolution of the spindle. This slight variation in the timing is largely due to variations in yarn thickness and is very convenient for observational purposes, because several stages of the slip can be seen stroboscopically at the same time. The loop which has slipped off can be seen floating away in a manner rather reminiscent of a lasso, when the yarn tension is moderate. The straightening of the loop is accompanied by a downward pull on the yarn, caused by its return to the bottom position of the coil, and this pull initiates a transverse vibration of the yarn in the vertical plane. A fore-and-aft vibration has also been generated by the release of the loop, and this is resolved into a transverse vibration of double the wave-length, as in Melde's experiment; this is the principal vibration. A minor transverse vibration is also started at right angles to the first one, by the side-slip of the loop across the spindle tip. Though the frequency of these vibrations is the same, they all differ slightly in phase.

The observer is left with a feeling of respect for the yarn structure which can endure this discontinuous twisting treatment, especially when it is repeated some four thousand times at the same spot, as in fine spinning.

The Mule-draw Effect.—The thinness of cottòn yarns is deceptive. Admittedly it would be possible to add a good many turns to one end of a stout hawser of hemp before any deformation of its twist was perceptible at the other end; the frictional resistances to the transmission of twist adjustments are evident in that case, but there is a smooth series of gradations from it down to the finest yarns. Reasoning on these lines I went to look for such an effect in mule yarn, and so made my first discovery in cotton-spinning, for the twist was appreciably greater near the spindle tip. An independent discovery of it was made by Oxley,[1] and has been described in detail.

The effects produced on the yarn by this " mule-draw effect " have already been mentioned as insignificant in comparison with the local variations due to drafting irregularities, but they are big enough to be easily detected, and it is strange that they were not recognised sooner. The simple method of holding up pieces of yarn with a weight attached, and counting the revolutions of the weight which happen before the yarn breaks, is quite good enough for demonstrating the twist-difference; a paper air-brake keeps the revolutions at a countable speed, the weight should be about a fifth of the normal breaking load, and some twenty separate chases should be averaged out. Each chase can be subdivided into a suitable number of parts, such as ten, and the position of the spindle tip ascertained. Though this simple method contained several unknown factors when I first employed it, subsequent studies have shown it to be surprisingly dependable.

In this way, or by more detailed survey with the Yarn Clinometer, it is found that the twist near the spindle tip may be 10% or more in excess of the twist near the rollers. This excess varies according to spinning conditions, and is an indirect measure of the power required to transmit the torsional stress along the yarn under given conditions of tension and vibration. The vibrations are, as we have just seen, very complex, and much assist this transmission.

[1] *Loc. cit., v.s.*, p. 94.

The effect is most easily examined by feeding the front rollers of the mule with two continuous filaments, such as silk, which are thus doubled together. The twist of these can easily be measured at any stage without much trouble or sampling error. The mule-draw effect on twist is then seen to be most striking when the mule carriage is stopped just before twisting at the head. This can be correlated to the fact mentioned previously,

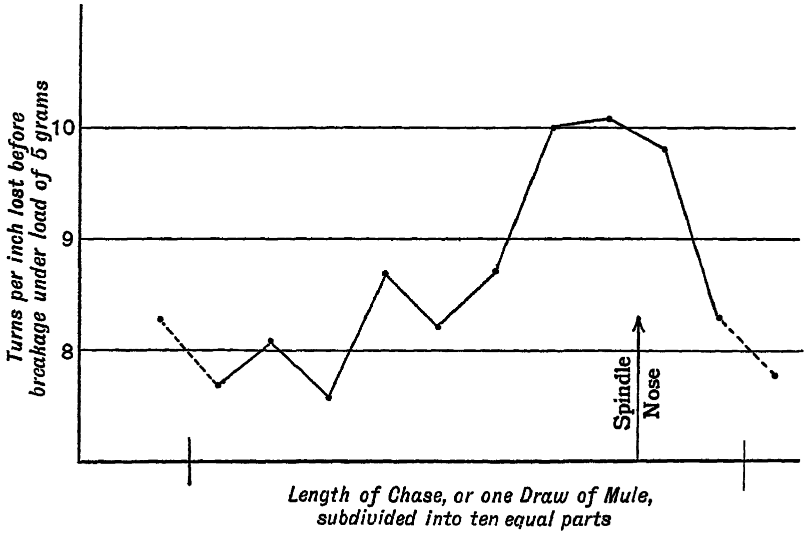

FIG. 41.—The Mule-draw Effect, demonstrated by the simple method of suspending a weight from the length of yarn under test, allowing it to untwist, and counting the revolutions to breakage. Data averaged from twenty chases off the same cop, each one divided into ten six-inch lengths.

The original average twist was 35 turns to the inch. Thus the actual twist-distribution along the draw, or chase, had been about 36 turns at the spindle tip, dropping to 33 turns per inch at the rollers. The parallel variation of "strength" was as 10 to 8 in this particular yarn.

that tension increases greatly during such twisting, and so helps the twist to shoulder its way along the yarn. Broadly speaking, the mule distributes its twist in a frequency which has its maximum at the spindle nose, and approaches the roller nip asymptotically.

Trapped Twist in the Ring-frame.—The significance of this phenomenon has been already indicated in connection with the ring-frame itself (p. 102). A casual observation gave the starting-point. Two differently coloured singles were being doubled on a ring-doubler when I happened to notice that the twist seemed to

stand still between the thread-guide and the rollers; each of the two brilliant colours was clearly visible, though they were being rotated over one another at more than a hundred revolutions per second. With a real double-screw thread this could only happen if it were moving endways through the double pitch in the same time-interval that it took to make one revolution; in other words, if it were revolving in a fixed nut. Hence it follows that in order to see these spots of colour standing still between thread-guide and roller at 10,000 r.p.m., turns must be added in very exact proportion to the length of yarn delivered; again in other words, the fixed number of turns which were trapped above the thread-guide when the spindle started have remained there unaltered, continually screwing themselves backwards along the new yarn arriving.

Therefore—and this was unexpected—the twist must be continually unscrewing from the thread-guide end. Thus, in a few inches of yarn which are being twisted up at these high speeds, there is actually a point at which it is being untwisted. This can be quickly demonstrated by reducing the trapped twist to zero, and keeping it so with a smooth rod, such as a pencil, inserted between the two parallel single yarns at the thread-guide; the two strands are then seen to creep over one another in two reversing spirals, the extent of this creep varying with the thickness of the pencil.

The amount of twist thus trapped is a constant for any given tension, yarn diameter, and coefficient of friction between yarn and thread-guide metal, under the given conditions of the spinning-room. Its demonstration in the actual spinning of singles on the ring-frame is necessarily less easy than with two-folds, but the same argument certainly applies, and accounts for the break-place distribution on the ring-frame, which has already been described.

Mapping the twist distribution on the ring-frame by means of two strands of silk, one of my colleagues found that the twist at any given instant was at a maximum near the traveller. From the traveller it diminished slowly towards the thread-guide, as in the mule-draw effect. But in passing from the lower side of the thread-guide to the trapped twist of the upper side, there was a sharp discontinuity and fall in the amount of twist, followed by a further drop over the remaining distance to the rollers.

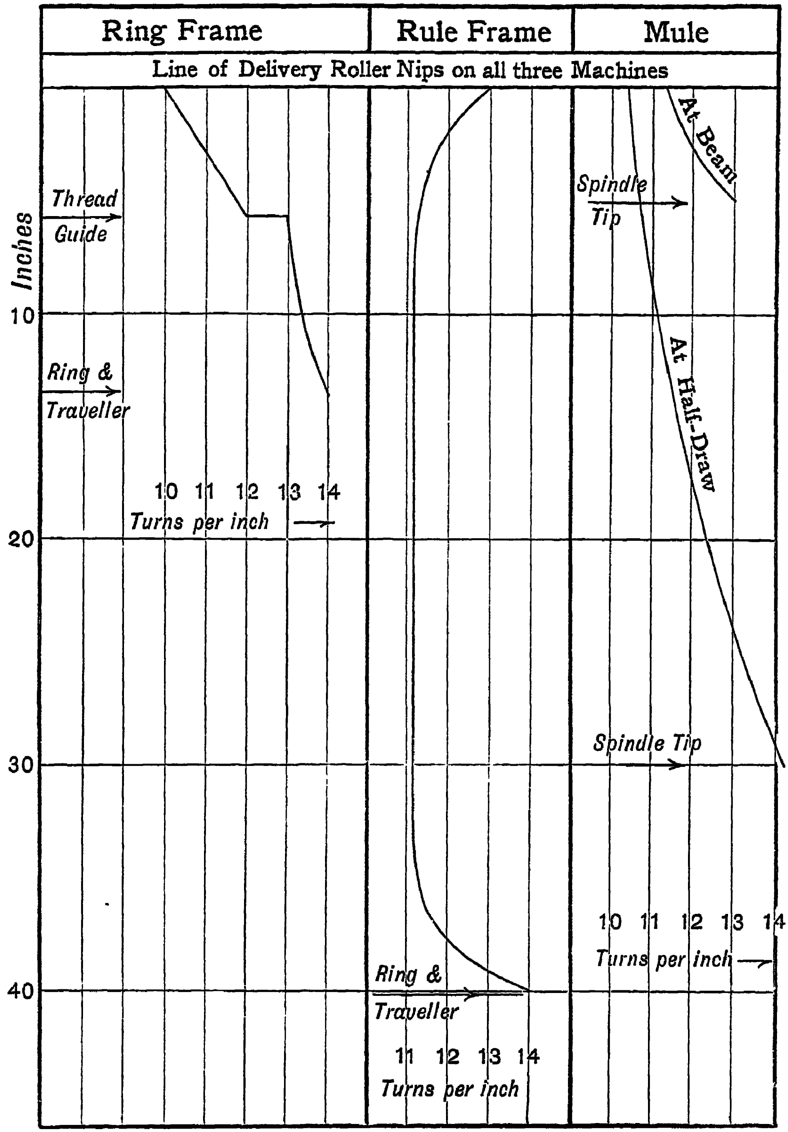

FIG. 42.—Diagrammatic distribution of the twist in three types of spinning machine.

Unique Distribution in the "Rule" Frame.—Both in the ring and in the mule the outstanding feature of the twist distribution is that the minimum twist is always found at the roller nip. In my hybrid experimental "rule" the distribution is entirely different (p. 103). The manner of the vibration of the yarn is circular, as in the ring-frame, but these vibrations are not damped by an intervening thread-guide; the yarn is thus left free to form a succession of nodes and loops, along which it traces a spiral path, passing at any given instant through two loops while completing one turn of its spiral. There is no constraint on this vibration until the roller nip is reached, and here the twist is found to be nearly as high as at the traveller, while the minimum twist at any moment is near the middle of the vibrating yarn. The reason for this very unusual bimodal type of distribution is not altogether clear to me, but the fact is very definite, and might well be made useful in spinning practice.

CHAPTER XI

HAIR ADHESION AND YARN RIGIDITY

THIS sketch of the principal features shown by yarn is now nearly complete enough for the purpose of appreciating differences in yarn strength. We have taken the straight and parallel hairs, slid them apart into a succession of drafting waves, and twisted them into a screw with a constant angle. It remains to consider the way in which this compound structure so holds itself together that its properties are also compound, being mainly the properties of the component hairs, and partly the properties of the construction as such.

There will evidently be a serial progression of these yarn properties caused by changes in the average twist, though this will be much complicated by the local variations of twist, and by readjustment thereof. It would be easier to study this progression in a perfect cylindrical yarn, or even in an imperfect yarn wherein twist readjustment did not happen. The former has never been made; the latter is actually available, for a two-fold is a structure wherein the readjustment of each component single is largely inhibited by contact with its companion.

The building up of yarn properties begins at the untwisted roving or sliver with such complete domination by the surface properties of the hair that this roving can be used to measure the latter, if the hairs are straight and parallel (p. 70). The importance of these surface properties is partly obscured by considerations of hair rigidity as the twist increases, the stiffness of many hairs being sufficient to offer an appreciable resistance to the torsional couple in low twists (p. 164). These are overcome in the higher twists, and the surface properties cease to act as limiting factors (p. 81) when the twist angle becomes sufficient to bind the hairs tightly enough to resist any load which the tensile strength of the hairs will carry; the variation of this point from cotton to cotton will be seen in the Thirteen Samples.

In such full and super-twists the rigidity properties of the hairs themselves become predominant.

Although the surface properties of the hair may cease to act normally as limiting factors in the higher twists, yet they still exist; they may even be modified as the result of the compression to which the hairs are subjected. They are liable to come into view again at any place or time when the twist is eased off by readjustment, by permanent set, or by wear. In the remaining chapters we shall find little occasion to consider any stage than that of full twist, at which the hair strength is predominant, so that the present chapter may usefully be devoted to the broader issues.

The Mutual Friction of Hairs.—The previous chapter discussed twist as if the adhesion of hair to hair were merely dependent on the twist angle (p. 169). Actually this adhesion under the pressure resolved from any given combination of yarn tension and twist angle and hair elasticity will depend on the surface properties of the hair. Whether such differences in adhesion will be visible in the properties of the yarn will depend upon whether or not the adhesion is a limiting factor in the particular yarn, under the circumstances in which it is being examined. A common example may be taken from the way in which yarn breaks; very soft yarns collapse gently and noiselessly by "slip," while the same yarn twisted more tightly will break suddenly with an audible "snap." The complications introduced by local variations in twist distribution are well illustrated by an intermediate stage, when slip-breaks and snap-breaks may happen indiscriminately during the testing of metre lengths.

In so far as we consider yarn in which the adhesion is still a limiting factor, which is not my primary concern in this book, so we must take the surface properties of the hair as a base-line for the yarn properties. In this connection it is worth noticing that one-half the surface of the outermost layer of hairs is always freely exposed to the air, and is not available for adhesion; as the yarn becomes finer the loss of surface in this way becomes quite appreciable, and assists in preventing the spinning of fine yarns from coarse cottons.

The extent to which the original hair surface may be modified by mutual pressure between the hairs, provided that a compressing force like the twist is available, can be well illustrated from the

Porometer technique, which I have already mentioned as a means for measuring changes in the cross-sectional area of the hair (p. 45). A length of sliver is drawn into a suitably proportioned hole in the metal plate, and cut off so as to leave the hole plugged with cotton; if the size of the hole is so adjusted as to give rather tight packing with dry sliver, this hole can be completely sealed up by the expansion of the hairs on exposure to saturated air. Such obliteration of the free air-ways between parallel hairs a millimetre long can only happen if they have so deformed one another that the cross-section of the plug of cotton is roughly like the appearance of a honeycomb.

Resistance to expansion of this kind will necessarily be less in yarn than within a hole in a metal plate, but the extent of this interlocking can be quite appreciable, for the yarn is a very dense structure. It can be compared with the densest bale of cotton on the market, and is much more dense than that. The steam-pressed Egyptian bale has a specific gravity of about 0·7, and nails can be driven into it as into wood. The density of any ordinary full-twist yarn as computed from its diameter measurements is around 0·8, so that it is much harder than the steam-pressed bale.

As a limit case of yarn we may take the double-spun, which is given a twist-angle sufficient to bind its wetted hairs into contact very securely, and the original surface properties of the hairs are largely smothered under their secondary deformations. At the other limit is the finishing draw-frame sliver, or untwisted roving.

Along these lines it may eventually be possible to trace some connection between the Convolutions of the hair (p. 19) and the properties of yarn, as displayed in differences from one sample to another, and apart from the fundamentally important increase in hair-flexibility which the bare fact of convolution provides. Personally I have not been able to find any confirmation of the common belief that their variations from sample to sample are of prime importance in spinning. It may well be the case that while an unconvoluted cotton would be too stiff to spin (p. 164), yet any commercial cotton is convoluted sufficiently, and none could be convoluted too much. If this is so, then although the convolutions are essential, they could never be more than a very minor limiting factor in yarn properties. But my evidence is purely negative, and as such is suspect; a search in the region of

low twists, whether of yarn or of sliver, may eventually show them to determine residual differences from one sample to another.

Two-fold Yarns.—The subsidiary industry of doubling has problems of its own, but, in so far as these are peculiar to it, they are simple in comparison with those of spinning; they are complex in so far as they involve the properties of the singles. Although the study of singles seems useless to many practical doublers, while that of the cotton itself seems even more irrelevant, yet it is actually the case that the hair properties are more clearly visible in two-fold than in singles (Chap. XII).

The conditions of adhesion between hair and hair are made much more stable by the act of doubling. Not only is the singles yarn no longer able to untwist itself bodily, but the local readjustments of twist (p. 172) are also prevented by the mutual friction between the two singles. The two-fold is thus a comparatively stable structure. It is true that this stability is only comparative, because the two-fold twist will also follow the spinner's rule in broad outline, and in its readjustments the twists of the constituent singles will play a minor contributory part, besides being themselves altered as a secondary consequence. Nevertheless this stability is far greater than that of a single, while that of a six-cord is practically perfect, as the seamstress knows.

The reduced variability of diameter, which is another consequence of folding two singles together, also assists to stabilise the two-fold. This reduction of variability is governed by the same statistical laws (p. 205) as the analogous case of the folding of slivers (p. 83). When six singles are thus folded into a six-cord, the result is sufficiently near to cylindrical form for all practical purposes, such as sewing.

The two-fold is of importance to my present thesis, in that it takes us away from the artificiality of singles. The single yarn is merely a convenient form in which the cotton is carried away to the next process, whether this is to be weaving, knitting, wire-covering, or cord-making. If the singles can survive these processes until their final incorporation in cloth, hosiery, insulation, or cord, then those twist readjustments which so largely dominate the behaviour of singles are practically inhibited; consequently, although it is most necessary to arrive at an understanding of the properties of singles yarn, yet such understanding

is only of indirect utility to the subject of this book; or, indeed, to any user of cotton.

The comparative simplicity of doubling problems is somewhat offset by the intricate solid geometry of two-fold twist. When the doubling is effected in the reverse spiral to that of the singles, the singles helix is itself coiled helically in such a way that any one hair comes repeatedly to the outside of the two-fold and repeatedly returns to be nipped between the two component singles. This would seem to be the essential peculiarity of two-folds, for the following reasons:

The resolved radial component of tension applied to a two-fold is much more uniform than the same component in singles of similar diameter and twist. The pressure thus produced in the "core of compression" of the two-fold cannot fail to be exerted upon every individual hair, and to be so exerted at intervals along each hair which are of the order of a millimetre only. Every separate hair is thus clamped to its neighbours in a score or so of separate places. Lastly, though most important, this secure and repeated clamping is just as secure under tension, whether the initial twist of the component single be low or high. The two-fold is thus a self-locking construction; it resembles those toys of basket-work which can be slipped on to the finger like a glove, but cannot be pulled off by mere force. Consequently the two-fold can be made soft and flexible for use in knitting, without losing its strength which—it should be carefully noted—can be that of the hairs themselves (Chap. XII).

Two-folds in which the twist is superposed in the same direction as that of the singles, instead of being opposed to it, need not be discussed in detail. It will be evident that they must show hybrid properties, nearer those of singles, though still characteristic.

The properties of two-fold yarns in respect of strength, flexibility, hardness, and shape in cross-section can be modified very greatly by varying the amount of twist in the original singles, as well as the amount and the direction of the two-fold twist. The foregoing comments on those features of two-folds which are relevant to my subject are based upon an experimental survey of doubling, for which some sixty checkerboard combinations of the two twists were spun and doubled, reaching out to absurd extremes. The results of this survey do not concern the reader,

except to note that the maximum strength obtained in some of these combinations was the intrinsic strength of the hairs themselves, as interpreted in the next chapter.

An interesting observation may be quoted in conclusion to show that even in two-folds there is need for caution when rigid treatment is being sought. One of my colleagues [1] discovered that when a two-fold was repeatedly twisted and untwisted, the amount of twist in the constituent singles could be altered. This queer result is attained in spite of the fact that both ends of both singles are firmly held all the time, and are finally restored to their original position. Turns of twist can thus be added or subtracted at will, by twisting one way more quickly than the other, and so controlling the time-factor in hair friction. The "perpetual motion crank" in cotton-spinning has always dreamed of an invention which would insert the twist between two fixed points, so that the yarn could be wound up without using spindles; I have tried to do it myself! In this observation we find the absurdity happening, and the explanation seems clear. The hairs can roll over one another in the loose-textured structure which is produced at certain stages of twisting and untwisting the two-fold; the movement is analogous to that of drafting, but takes place sideways instead of endways. The effective result of this is the same as if twist had actually been altered by rotating one end of the singles. The phenomenon is also interesting as showing that a fairly parallel hair arrangement does exist in singles yarn; although such "side-draft" is not likely to be conspicuous in yarn tests there is evidently a source of error in it, and it undoubtedly plays a part in the depreciation of yarn or fabric in everyday use.

Yarn Rigidity in Tension.—The common usage of the industry is rather confusing in this matter. The total Extension of the yarn is not clearly separated into its two components, Elasticity and Permanent Set; the "elasticity" of common use is mainly the total extension, while true elasticity is judged as "feel."

Apart from the confusion of words, this lack of discrimination is largely unimportant, because of the importance of the Time Factor in measuring these quantities; the permanent set increases continually with longer and longer exposure to tension. Having regard to the composite structure of yarn, this is not surprising.

[1] Miss E. Lanigan.

The molecular properties of the chains of cellulose micellæ in the fibrils (p. 23) are modified by the cushion of air and water between them (Chap. III), and are further influenced by the reversing spiral arrangement; the resultant of all this is further modified by the perfection or otherwise of the hair-to-hair adhesion in the yarn (p. 183); on this again are superposed the effects of twist readjustment (p. 172), together with the fact that stress is locally concentrated by the enormous variations of cross-sectional area (p. 160). In fact, there is no such property of yarn as its elasticity or its permanent set, as such. There is only a complex of phenomena, which may eventually be sorted out into the component factors; even the "tension" with which this section deals is not pure tension, but includes resolved stresses in directions other than that of the yarn axis.

Under these circumstances any test must be to some extent arbitrary, and to meet them I devised the Elastometer (p. 351), which exerts no inertia effect while straining the yarn at a standard rate of 0·1% of its length per second. The rate of loading is left dependent on the strain of the yarn, as in the lea tester. The load is applied in steps, with a special precaution ensuring that the yarn cannot be accidentally extended beyond the length it had previously reached under load. The Time Factor is eliminated, apart from the fixed rate of strain, because the yarn is released entirely from its load as soon as the extension allows this load to be lifted. Readings taken after each step of the load has been applied give the total Extension and the Permanent Set, the Elasticity being obtained by difference.

Hysteresis measurements can be made with this instrument by modification, but it has advantages in normally eliminating hysteresis, because the variation from one strand of yarn to another is excessive in this respect, as a consequence of twist readjustment. For comparison of yarn of different counts the readings must be corrected approximately by expressing the load in terms of the relative average cross-section, which is inversely as the count. The standard length of yarn tested is one metre.

The pivotal fact disclosed by this separation of elasticity from permanent set can be seen at a glance by inspecting the curves obtained from continuous filament yarns (like the artificial silks) in comparison with yarns made by spinning from the same filaments cut up into short lengths like cotton, and these again in

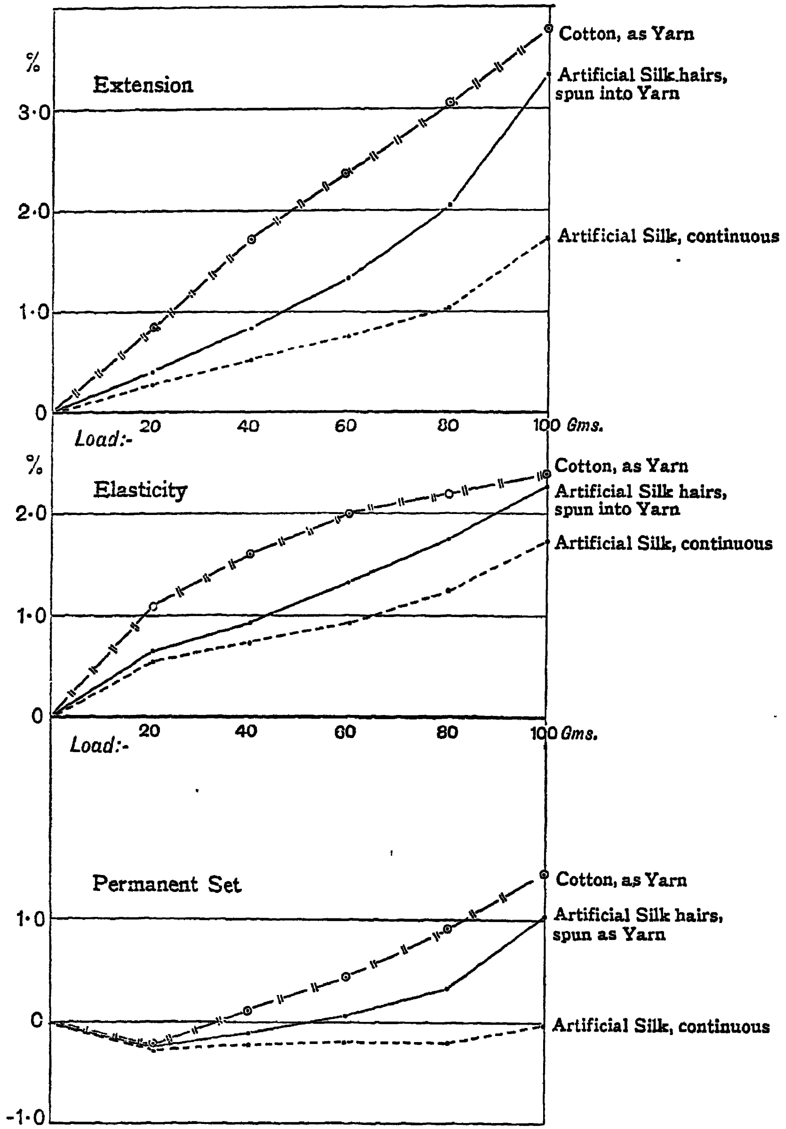

FIG. 43.—Comparisons made with the Elastometer between the tensional rigidities of three yarns spun to similar counts and twists, but constructed differently.

The properties conferred respectively by the raw materials, and by the construction of spun yarns from separate hairs, can thus be separated.

comparison with the cotton yarns themselves. The rigidity phenomena shown by the continuous filament yarn are evidently in the main those of the filament itself, while those of the spun yarn built up from the same material are quite different. The yarn structure dominates its properties in the latter case. Thus, the permanent set of an artificial silk was negligible in the continuous filament; when built up into spun yarn the same silk had a permanent set like that of cotton.

It will not be too sweeping to make a rough distinction as follows: the elastic properties of a spun yarn are mainly those of the constituent hairs, while the permanent set is mainly a consequence of the yarn structure.

From this starting-point the reader may turn to the Thirteen Samples, which show striking differences among themselves (Chap. XIII). Here it might seem that all these differences must be due to the cotton, since the counts and twists of the yarns were all the same. While this must ultimately be the case, yet the connection is partly indirect, because the individual yarns have different structures as a consequence of being thus made from different cottons. Such structural differences can be located in the drafting waves, the deviations from the spinner's rule, the number of hairs in cross-section, and in the hair surface affecting the adhesion. Any of these would affect the rigidity of the yarn, but the details of the way in which they do so have still to be worked out.

A side-issue of minor interest is found in the "Negative permanent set" which all yarns display on the Elastometer to a greater or less extent. After a first light loading and release, the length under test becomes shorter instead of being lengthened. In two-folds this negative set is not easily obliterated, even after heavy loading subsequently. It is due to the sudden release of the yarn from tension; this acts somewhat like the cracking of a whip, shaking out the hairs from the close contact in which the spinning had bound them. The two-folds show it more emphatically because their hairs are more closely interlocked by wet doubling.

One other example of the use of the Elastometer may be quoted, in the effects produced by altering the twist of single yarn. On using it to test a series of yarns which were spun with progressively increasing twist factors (p. 169), there is seen to be a

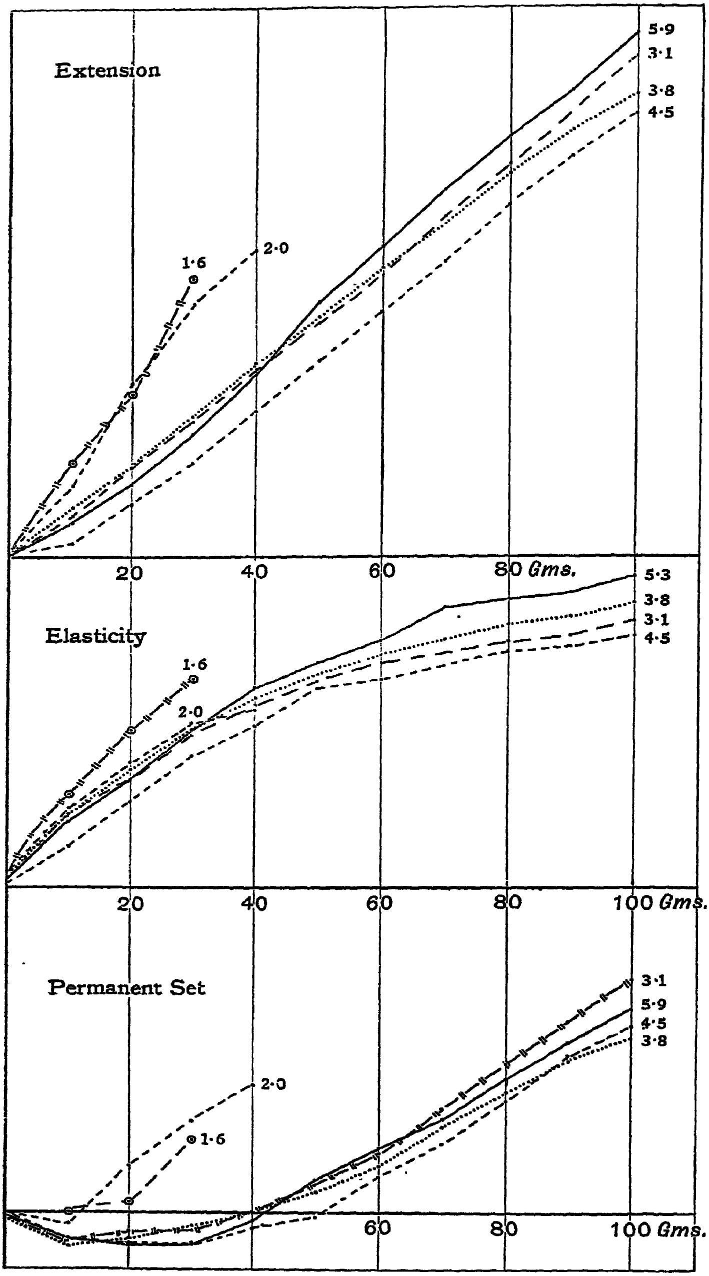

FIG. 44.—Effect of twist on the tensional rigidity of yarn, as examined by the Elastometer on the same yarn spun with different twist factors.

sharp discontinuity in the type of rigidity displayed. This discontinuity occurs when the twist angle has become big enough to resolve enough of the tension into that pressure between the hairs which prevents slip (p. 172). The differences in hair-surface properties from sample to sample can be partly examined by this means. It is also noticeable that although further increases of twist beyond this point have some effect in postponing the loading at which permanent set begins, yet it cannot be prevented entirely by ordinary spinning; this is shown with greater accuracy when the additional turns are added to yarn which is already in position on the Elastometer, and is kept under constant load meanwhile.

The effect of the length tested upon the extension shown is noted elsewhere (p. 266).

Yarn Rigidity in Torsion.—It is better in many respects to use reciprocating stresses, rather than dead loads, when the properties of such a material as cotton are being investigated. The interpretation of reciprocating torsional stresses applied to yarn would, however, be very difficult, because the singles yarn has a unilateral structure with respect to torsion. Some facts obtained by the ordinary static method will therefore suffice.

The observations were again made on one-metre lengths, in a simple Torsionmeter (p. 363), the load on the yarn being adjustable. Turns of twist could be added or removed, and the torsional reaction of the yarn was observed on an indicator, while the contraction or elongation of the original length could be also observed.

It was found that the torsion rises in direct proportion to the number of turns added, and shows an enormous hysteresis loop when these added turns are removed; the curve drops almost to the original value as soon as untwisting begins. This is due to the permanent set of the length of the hairs, under the resolved stress, in combination with the same unpacking effect which has just been cited as the cause of "negative permanent set" on the Elastometer.

The contraction of length is logarithmic, becoming quicker in the higher twists, and also showing hysteresis. Here the hysteresis is very much smaller than for torsion, so that the yarn is only slightly longer when brought up to some particular twist than it is when brought down to the same state.

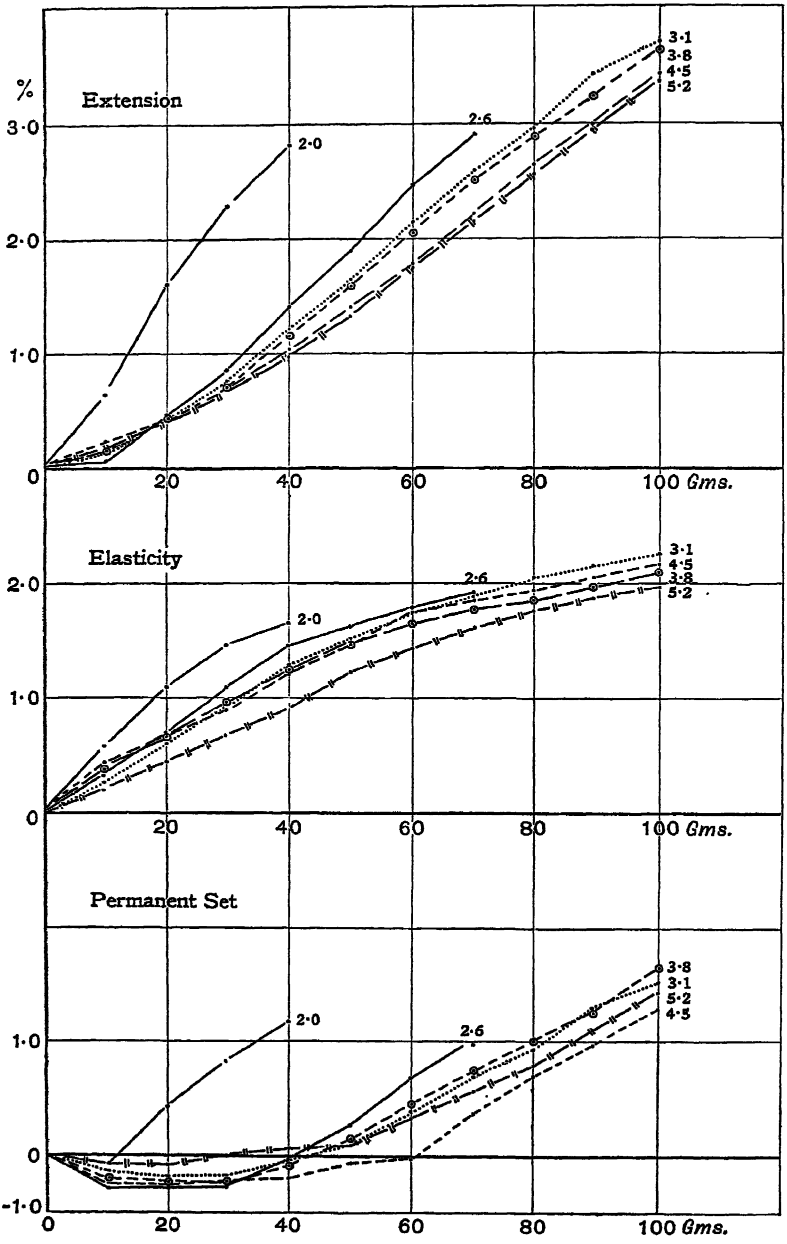

FIG. 45.—Effect of twist, as in Fig. 44, when the various additional twists have been added subsequently to each length before testing, under constant load. The original yarn had 2·0 twist factor. The substantial identity with Fig. 44 is of interest.

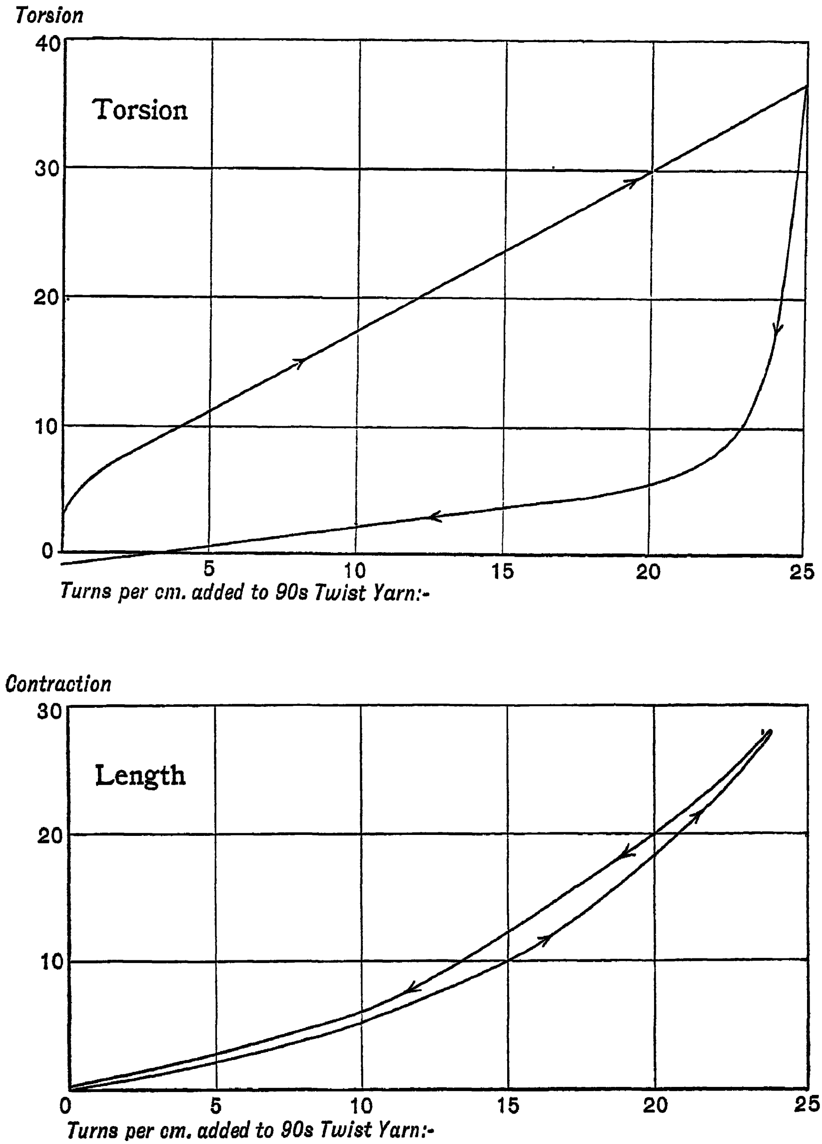

FIG. 46.—Typical observations to show the effect of adding twist to a yarn and removing it again, in the alterations of Torsion and Length thus produced. The yarn originally contained 14 turns per cm. in 90s count, and was tested in one-metre lengths, under a load of one gram.

The initial torsion exhibited by various yarns of the Thirteen Samples was such as to indicate that zero torsion would correspond to a twist factor of about 1·5. This corresponds broadly with the

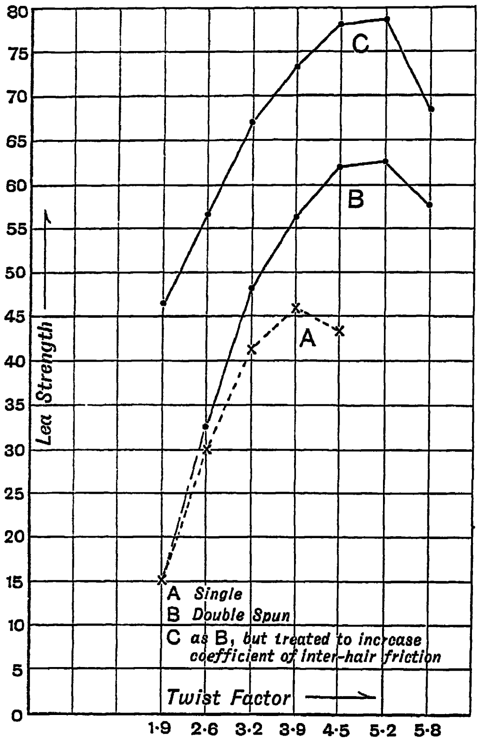

FIG. 47.—The inter-relation of Twist, Adhesion, and Strength in singles yarn.

Curve A shows the increasing " strength " of a yarn spun with increasing twists, its optimum being at 3·8 twist factor.

In Curve B the hair-to-hair adhesion is increased by wet double-spinning of the softest single, thus adding turns up to the same total twists as in A. The optimum rises to 4·9 twist factor.

For Curve C the same treatments as in B were preceded by an artificial modification of the hair surface, after the soft single had been spun. The coefficient of friction was thus increased, but the optimum twist is unchanged, though the " strength " is raised throughout.

results given by the same yarns in the Twist-Yield tests (p. 175), where they broke under the lightest practicable loads when the twist had been reduced to this amount.

Under fixed conditions of twist it was found that the torsion

exerted was proportional to the load applied to the yarn, thus expressing the resolution of the tension by the spiral structure.

These elementary experiments with the Torsionmeter conclude my preliminary account of the general properties of yarn. The reader should now be ready to pass on to the final stage, following up all the threads of evidence from the previous chapters to their convergence in the next chapter upon my interpretation of the meaning of " Strength " in yarn.

CHAPTER XII

THE STRENGTH OF YARN

WITH the subject of this chapter the nucleus of my topic is reached, and its title would seem to be free from ambiguity. Yet in reality it is so ambiguous that to describe a yarn as having a strength of 100 lbs. in the lea test is no more informative than it would be to describe a railway as "one which passes the 1000-ft. contour line after one mile." This information would apply equally to the rack railway on Snowdon or to the ordinary railways round Buxton. The railway engineer will want to know the altitudes and locations of the starting-point and of the summit, hence obtaining the average gradient, at the very least.

This analogy is in no way far-fetched. It will presently be shown that there is a Strength Gradient in yarn, determined by the distribution of weak links. There is also a Peak Strength, which cannot be greater than the Intrinsic strength of the hairs (p. 67), less the small component resolved in radial pressure (p. 172), and may be much less than this if hair adhesion is a limiting factor (Chap. XI), through inadequate local twist (Chap. XII).

Thus to begin this chapter by asserting that "strength of yarn" is a meaningless phrase, in itself, may appear to the reader as mere preciosity. The strength of yarn is indicated by a figure obtained in the lea test; this figure is known to be dependable, and to have practical usefulness; moreover, as will be seen presently (p. 233), this figure has a sufficiently definite scientific meaning. But my criticism of the expression has nothing to do with the use of any one testing method; it is due to the fact that two yarns can show the same "strength" in some standard test, and yet may possess very dissimilar strength properties. The two yarns can reach the same result by two different ways. That is one reason why single-thread tests do not always agree with lea tests.

Or, again, the reader may object that even if this is so, still

the strength which is shown by a lea test is the working strength, so that an exact definition is immaterial, apart from my personal interest in connecting the properties of cotton with the properties of yarn. But this also is irrelevant, because—as already noted—only a portion of the properties of singles yarn is ever available in the finished cloth, hosiery, or cord; whereas the spinner's strength tests are made on singles.

The expression " strength of yarn " must therefore be employed for the future on the understanding that it is a loose expression, covering a complex set of strength phenomena, whereof the principal constituents are the Strength Gradient and the Peak Strength. But before proceeding to a detailed discussion of these constituents there is one criticism which must be made upon the work of a predecessor; it is unavoidable, because his error has so hampered all his successors, including myself.[1] To make it at this stage, and so be shut of an unpleasant duty, a brief preliminary explanation is required.

The abundance of weak links present in any particular piece of yarn under test can be altered as the experimenter may desire; the longer the length tested, the more weak links should be present, and—on the average—the weaker the weakest link should be. Consequently, the yarn should give weaker test results in long lengths than in short ones. Moreover, since this relation between length and strength should be a matter of average chances, it ought to give information to the statistician concerning the method of distribution of these weak links; this will presently be examined. Meanwhile it follows that if the length tested could be shortened down to some small amount, such as 1 cm., the average strength should be much higher than when the tests are made in lengths such as 100 metres. This is so because in the latter case the strength shown should be that of the weakest link present in the whole length; in the former case, when the whole length is tested in 10,000 separate centimetres, the weakest result should be the same as that given by the same weakest link in the hundred-metre test; therefore, every other one of the 9,999 centimetres should be stronger than this one, and some of these should be very much stronger because such short lengths should isolate the strong links as well as the weak ones.

[1] W. L. B., *Raw Cotton*, p. 146.

For the moment the reader can neglect the recurrence of the word "should," merely noting that it is used because subsidiary experimental difficulties prevented me for many years from reaching my proof that this common-sense interpretation is also the correct one. Things do not happen quite like this in actual tests of yarn, but this is the real state of affairs underlying the observed facts.

Now, if the strength of yarn at 1 cm. must inevitably be greater, even very much greater, than its strength at a hundred metres, and if the lea test is presently shown (p. 233) to be practically identical with tests made on a length of 120 yards, what becomes of that calculation which has been copied for nearly half a century? It shows that "we utilise very little more than 20% of the total strength of the raw material, whether it be American or Egyptian cotton, when it is spun into single yarns. . . . In two-fold yarns the results are rather better by about 6%." [1] It is comparing things which are incommensurable, for the hairs of the raw material were certainly not tested in lengths of a hundred and twenty yards, but the yarn was.

The statement thus is meaningless. It is as if one should say that the L.M.S. Railway only utilises 20% of its resources because it climbs to 150 feet above sea-level at Crewe, whereas Shap Summit is over 800 feet high. Also, it has been prejudicial to industrial research on cotton, because it has engendered the impression that nothing useful could be ascertained by trying to measure the properties of raw cotton, yarn construction being shown to be so inefficient; only 20% efficient. But this conclusion did not tally with instinctive knowledge, which felt that things could not possibly be as bad as this, and tried to find excuses for the yarn, blaming it on the cotton because the yarn seemed to be so well known (p. 77).

Thus one authority writes: "Using the table for the calculated strength of spun yarns, and comparing this result with the actual strength given on the testing machine, is a reprehensible practice, and can only lead to the fixing of false ideas on the subject in the minds of the readers; loss in strength through the presence of immature and over-ripe fibres, distortion of the fibres when in the aggregate as yarn, and the absolute impossibility of obtaining equality of tension in each fibre, are factors inherent in all yarns

[1] Bowman, F. H., *The Structure of the Cotton Fibre*, Manchester, 1881, p. 143.

and cannot be obliterated; they alone represent a large loss in strength in any test, and consequently it is scarcely correct to speak of the theoretical strength of yarn."

To such depths of agnosticism has this meaningless calculation made by Dr. Bowman in 1881 reduced the author of a leading book on cotton-spinning, in his eighth edition.[1] The extenuating circumstances which it drives him to plead are in reality unimportant; it has already been shown that such abnormal hairs must be included in measurements of hair strength (p. 67); that the distortion in yarn, even at surface twist angles of 25°, must be quite small (p. 117); that the tension becomes effectively equal on every hair in conditioned yarn through permanent set so that the effective and bodily diameters of yarn are normally identical (p. 172). I shall presently show that twist yarns do commonly attain to the "theoretical strength," when the theory has been made reasonably complete, while the lower strength of soft yarns (p. 184) is also within the grasp of theoretical prediction.

I incline to think that this one statement by Dr. Bowman is, by itself alone, responsible for holding back the scientific study of cotton-spinning during several decades; such, though for a shorter period, was certainly its effect upon me personally. The urgency of the need for its removal from the literature is such as to warrant me in assisting that removal by pointing out that not only are these calculations without real meaning, but that they are also wrong. The error in them was detected by Mr. McConnel; it consists in having calculated the load which the hairs might carry in a lea test as if it were borne by eighty strands of yarn, one for each revolution of the standard wrap-reel. But each such revolution makes two strands when the lea is placed on the two hooks of the tester. Therefore, all the "calculated lea strengths" should be doubled, and the efficiency calculated at 20% must accordingly be reduced to 10% only. In any case, the calculations are made from O'Neill's scanty hair-strength figures,[2] and not from the hairs used in the yarns tested.

Finding the reputed efficiency of cotton and cotton-spinning thus cut down to a mere 10%, my reader will no doubt concede that a re-investigation of the situation, starting from the beginning, is worth making.

[1] Scott Taggart, W., *Cotton Spinning*, London, 1924.
[2] Bowman, *loc. cit.*, p. 81.

Introductory.—It is essential for the reader to get to grips with the conception of a Strength Gradient as soon as possible, so that this will be first discussed. The component factors of the actual strength found locally while testing for this gradient can be taken next, in the process of leading up to the special case where local strength has been followed up the gradient to the imaginary quantity which I have denoted as the Peak Strength. The actual making of strength tests can be taken last of all, beginning with sampling precautions, and corrections for parallel determinants, leading up to the analysis of the lea test.

Throughout the subdivisions of this chapter there must be continual reference to a strength phenomenon which cannot conveniently be isolated in any one of them, namely, the effects which are due to Readjustment of Twist. This readjustment has been discussed in its own chapter as an outstanding peculiarity of singles yarn (p. 172), and its effects can be traced throughout the study of strength in such yarn, because it modifies and renders unstable the structure which originally existed therein. These effects are influenced by a time factor, and they also play an important part in the peculiar experimental difficulty which I shall term the " clamp effect "; they disappear almost completely in two-folds, so that a direct application of weak-link interpretations can be made upon two-folds, and then taken back to explain the singles.

Therefore it will probably be easier for the reader in the long run to begin by reminding him again (pp. xvi and 78) that singles " yarn " is not a static body, but is to a large extent a vague and shifting complex, whose strength it is correspondingly difficult to define. The degree to which these ephemeral alterations control its strength will necessarily vary with the method by which the strength is tested; thus the Time Factor in twist readjustment will be negligible in impact tests, appreciable in dead-load tests, and maximal in repeated-stress tests.

To conclude these introductory and cautionary remarks I would lastly redirect the reader's attention to the enormous variability of yarn. This we have seen measured as yarn diameter (p. 128), and squared to express the cross-sectional variation (p. 159); just as the variability of hair break is greater than that of hair cross-section, so the variability of yarn break exceeds that of its own cross-section, because other variable factors are involved. Therefore a blind testing for strength by

repetition of a single test requires prohibitive expenditure of time and labour; the Probable Error of a single-thread test is of the order of 10% and upwards, so that at best the P.E. is still 1% for a hundred tests and 0·2% for the average of two thousand five hundred tests, because the significance of the results follows the square root of the number of tests made. The large experimental errors thus involved can only be evaded by measuring concurrent variables, and applying corrections accordingly, but to do this intelligently we must understand how our corrections operate.

The Strength Gradient

The "gradient" of strength has already been described (p. 83) as the change in strength which follows upon a defined alteration of the length tested under any given method of testing, such as dead-load in single-thread. It may be described alternatively as "the length-strength relationship."

Setting on one side for the moment the complications introduced by Readjustment and regarding a length of yarn as being a static assembly of successive links whose strength is very variable, the rate of change in strength for any given change of length tested will follow statistical laws. For my knowledge of these laws and their consequences I am very greatly in the debt of my friend Mr. G. Udny Yule, F.R.S., who interested himself in the problem at a stage when I had not succeeded in overcoming the difficulties of interpretation which obscured the reality lying behind experimental evidence. The statistical basis which he then provided lay unused for some seven years longer, till this interpretation became possible by its employment.

Mr. Yule's study of the results to be expected from the breaking of a chain, whereof the strength of the links is varied in such a way as to give a completely "random distribution" of individual link-strengths, comes to the following conclusion:

> "If W_x is the proportion of samples of unit length which give a greater break than x,
>
> and
>
> If nW_x is the proportion of samples of n units length which also give a greater break than x,
>
> then
>
> $$nWx = (W_x)^n.$$

Thus one can calculate the deviation of the *median* break of length n from the median break of the length l. We can assume provisionally that the distribution of the breaks at unit length is a normal one. The results of such calculation show three general characteristics:

(*a*) Break falls off less and less rapidly as length increases.

(*b*) Breaks are more variable for short lengths than for long ones.

(*c*) The distribution becomes unsymmetrical as length increases, with a tail of weak breaks."

The experimental verification of these predictions will be taken shortly. Meanwhile especial attention should be directed to Mr. Yule's clause (*a*), for this is the theoretical basis of the phenomenon of the Strength Gradient. The gradient is more conveniently discussed by the statistician as a falling-off with increase in length, but the cotton-spinner will be more likely to look upon it as a rise with decrease of length, starting from the lea test value. The actual form of this slope is a logarithmic curve, so that the strength changes by the same amount for every halving or doubling of the length under test. This is very handy, because if the tests are made upon chains of links which have such lengths as $\frac{1}{2}$, 1, 2, 4 and 8 metres, the resulting curve will be a straight line which has a constant gradient through points plotted on equidistant ordinates. This gives a simple definition of the Strength Gradient, which now becomes the "halving-change," the amount by which the strength changes when the length is halved; it is less confusing to speak of "halving" than of "doubling," on account of the technical meaning of the latter word.

Though this discussion refers to real or imaginary chains of links, yet it also applies to actual yarn, provided that the "links" which exist in the yarn when the test begins are still relatively the same when the test ends. Readjustment of twist (p. 159) defeats this proviso in singles, especially in bad singles, but the proviso applies quite well to two-folds. The act of making our singles yarn into two-fold is not, however, a mere prevention of readjustment, because it incidentally also effects a reduction in the variability of the yarn, and so modifies the distribution of weak links, and hence the Strength Gradient. This is not a

serious difficulty, however, for it can be corrected by calculation; again I am indebted to Mr. Yule:

> "Let S be the standard deviation of either constituent single, and M the mean break of the same.
>
> Then for the two-fold the corresponding values are $\sqrt{2}S$ and 2M.
>
> Thus the relative variability of a two-fold (standard deviation, coefficient of variation, or probable error) is that of the single multiplied by 0·707."

We can now turn to examine the gradient in actual yarns, beginning with the simple case of the two-fold, afterwards returning to the more complex singles. It should be noticed that the actual values usually plotted as five points on my routine arrangement of the gradient diagram are themselves the averages (more strictly, the medians) of the five frequency-distributions of individual breaks obtained at each length. Therefore it will be better to examine the actual frequency-distributions, because these will show the details of the way in which this halving-change is effected. It need hardly be added that my choice of this particular series of five points at 8, 4, 2, 1 and ½ metre lengths is simply a matter of practical convenience in making the tests (p. 205), while my reason for not using lengths which are shorter than ½ m. will appear subsequently in connection with the "clamp effect" (p. 215).

A suitable set of data can be taken from the Thirteen Samples by throwing together all the tests on all the samples at each length; this makes sure that any effect we may find is a typical property of yarn as such, and not the peculiarity of some one yarn. The actual tests were made serially along each cop, the cycle from eight-metre to half-metre being repeated several times; this averages out local variations in the yarn, and so it becomes feasible to attempt to predict the strength of any one length from that of the shorter lengths. Thus I worked through the list of half-metre tests in the order in which they were made along each of the fifty-two cops, and noted the weaker of every two successive breaks; this is simply to assume that if the full metre had been tested, the break would have been that of this weaker member of its two half-metre links. In this way the half-metre tests are used to provide a forecast of what the one-

metre results might be, if the simple weak-link assumption held good.

Similarly, I took the weakest of every successive four half-metre values to predict the two-metre result; and again, the

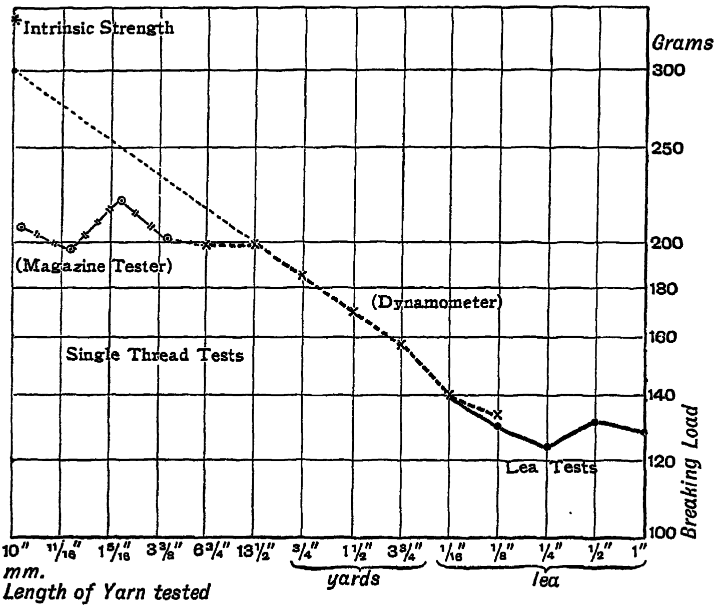

FIG. 48.—The principal phenomena of Yarn Strength as shown by a super-quality singles yarn.

Diagram showing the rising gradient of strength from one-eighth of a Lea to 13½″, in six halving-changes. Extrapolation of this gradient to 1 cm. defines the Peak Strength at 300 grams. The Intrinsic Strength, as obtained by measuring the hair break and hair weight, disregarding the small deduction for resolved radial stress, is shown at 348 grams; this unusually small deficit of 14%, making the Peak Strength almost identical with the real Intrinsic Strength, is characteristic of real super-quality.

The Clamp effect in depressing the result of short-length tests further and further below the true gradient is seen in some four halving-changes between 6¾″ and 1 cm. The latter at 209 grams should be compared with the true hair strength tested in the same length at 348 grams.

Extrapolation of the gradient downwards coincides with the strengths inferred from Lea tests on fractional leas, but the Packing effect produces a false gradient which raises the full Lea test to 130 grams instead of 105 grams, a difference of 25 grams.

Date from full-twist yarn of only 55s count spun down from fine cotton.

weakest of eight successive half-metres to predict the four-metre frequency curve. My data were not extensive enough to go beyond this, so that the process was repeated with the one-metre

tests to predict the eight-metre, besides making additional predictions about the two- and four-metre, and so on.

The actual graphs thus obtained can be consulted. At each length is given a frequency-distribution for strengths actually tested at that length. Inside this lies another curve containing only half the number of points, which is the weak-link prediction made from pairs of the next shorter length; inside this again is a curve containing only a quarter of the original number, predicted from the groups of four tests obtained in a quarter of this original length. And so on, like an Easter egg of concentric shells (p. 251).

Considering first the results in Two-fold. The frequency-distribution of breaks at half-metre length is somewhat skewed; as the length tested increases, so this skew is diminished; in the words used by Mr. Yule (p. 204) in his conclusion (*c*), there is a tendency to form " a tail of weak breaks." The variability in short lengths is evidently greater than in long ones, when the half-metre is compared with the eight-metre, thus verifying his conclusion (*b*). The fall in median strength from one length to another is practically constant around ten grams, and as these lengths differ by successive multiples of two, not arithmetically, his first conclusion (*a*) is also substantiated. The sets of concentric graphs at each length can now be examined in order to verify conclusion (*a*) in detail; it will be seen that the four curves shown for tests at four-metre length all fit neatly inside one another, and have the same centre, or mean. Thus the strength of these two-folds in four-metre length can be ascertained just as correctly by any one of four methods; either by testing in four-metre lengths directly, or by taking the weaker of each successive pair of breaks obtained in two-metre lengths, or the weakest of each successive four in one-metre lengths, or the weakest of each successive eight in half-metre lengths.

Thus, with these particular two-folds the theory of the weak-link distribution accounts entirely for the observed phenomena of the Strength Gradient, within the error of actual measurement. This is a very firm starting-point from which to return to the study of singles. Before doing so it should be stated that these particular two-folds were doubled from the ordinary full-twist singles, with the same number of turns applied in the reverse direction.

Turning next to the singles, tested as such, deprived of any

companion, and therefore able to undergo free readjustment of twist; remembering also that these were poor yarns, spun too fine, and therefore likely to display any readjustment phenomena rather more easily than good yarns. They differ from the two-folds very markedly, but before examining these differences in detail it is advisable to consider the difference of variability which doubling causes; it has been stated (p. 205) that the variability of the two-fold stands to that of the single as 0·707 to 1·000. Therefore the strength gradient of the more variable singles should be much steeper; the halving-change should be nearer fifteen grams than the ten grams given in two-fold. Actually the opposite is found to be the case; the halving-change is only about four grams. Seeking the cause of this breakdown, and taking the three conclusions one by one as before, we find no decided change in the form of the frequency-distribution between half-metre and eight-metre; the increased variability in eight-metre is still visible, but much less decided than in the two-folds; the halving-change is still fairly steady, although much smaller than it should be. Turning to the details of the latter in the concentric graphs, it is evident that these graphs are no longer concentric; the inner ones huddle over to the weak side of the outermost one. Thus, when we apply the weak-link method of prediction to singles yarn, the prediction always indicates that the yarn *should be weaker* than actual experiment shows it to be.

Thus, with these particular singles the theory of the weak link breaks down. Always it is the case that the yarn is stronger and less variable than it ought to be. This is a most useful property of singles; indeed, it is permissible to doubt whether many of the things which can be done with long lengths of singles could be done at all, were it not for this habit of concealing its own weak links. For that is what these graphs indicate; measurement of strength in a short length shows that certain weak links are present, but when a longer length is tested it is found that these weak links strengthen themselves. That this explanation is the true one will, I think, be conceded when the results of testing very short lengths have been noticed.

But the necessary machinery for thus strengthening the weak links in singles has already been examined in Chap. X as the readjustment of twist. This in its turn is a consequence of the

irregularities of yarn radius due to the drafting waves. Therefore it may be reasonably inferred that the worse the yarn, the more it can develop this self-adjusting power. The better the yarn, in the sense of regular diameter, the less self-adjustment it can display: a remarkable consequence of this will be seen in the Thirteen Samples, whose relative halving-change in singles is roughly the reciprocal of that in two-fold.

Consequently there seems no escape from the interesting conclusion that any testing of yarn strength in singles, and especially in the form of single thread, tends automatically towards obliterating the difference between a good and a bad yarn; the observer's scale readings are cramped.

A further inference is important in taking us away from the artificiality of singles. Since the strength displayed by singles in ordinary mill tests, such as the lea, is very largely the result of its self-adjusting capacity, and further, since readjustment is inhibited when the singles are built up into cloth, hosiery or cord, it follows that the differences in strength which are shown by the finished articles will be proportionately greater than the differences shown by lea tests. I think I am justified in claiming that this conclusion does actually fit with practical experience; the 30-lb. yarn makes twice as good an article as the 24-lb. yarn, when the difference is only in the cotton used.

Thus, the fact that singles yarn is self-adjusting, and gives test-results which are better than its real strength, is no argument for remaining satisfied with faulty yarn. And it should be remembered that all yarn is faulty, as my reference to super-quality aeroplane yarns when discussing drafting (p. 126) will sufficiently illustrate.

Having established the reality of the Yarn Gradient, and seen that it is fundamentally a matter of weak-link distribution, much modified in singles by readjustments, it may now be utilised to define the differences between two yarns. If two yarns have the same gradient, then their relative strengths in all lengths will stand in the same ratio. If they have different gradients, then there should be some one length at which both will show the same strength; if this point of intersection happens to lie near the lea length they will be described as having the same lea strength, while the results of single-thread testing on the half-metre machine which is commonly employed will disagree with the lea tester.

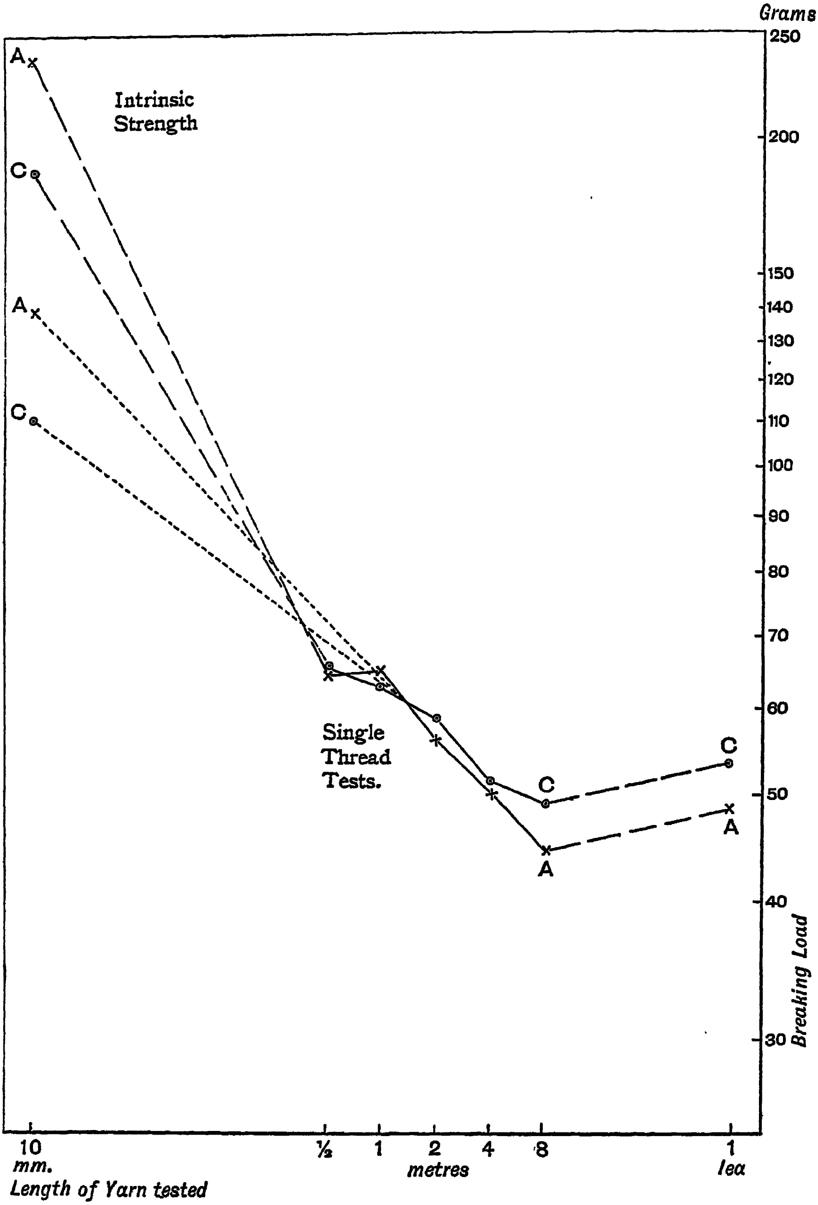

FIG. 49.—The Crossing-over of Yarn Strengths, shown by comparing two different cottons under exactly parallel conditions of spinning and testing, on account of the existence of differences in gradient between their yarns.

But this is a very common event. Even within the Thirteen Samples the gradient differs so much from one sample to another that the halving-change is less than 4% in some and nearly 8% in others (Chap. XIII). With differences of this order accumulating between the successive halvings of 120, 60, 30, 15, 7½ yards, and still nearly four more halvings to reach the half-metre, the existence of discrepancies between lea and half-metre tests is no matter for surprise; agreement would be more surprising.

The flatness of the gradient in some single yarns is remarkable. One of the Thirteen Samples which rose from 140 grams at lea length to 240 grams at the half-metre when examined in two-fold, as the pure effect of weak link distribution, and should therefore have undergone some such change as 1 to 2 in singles, actually altered only from 46 grams to 56 grams, or 1·0 to 1·2! These yarns were, as I have said, an extreme case of the importance of readjustment in thus flattening out the gradient. Indeed in the case of one of them this flat gradient, combined with the "clamp effect," produced the experimental result of showing the same strength in singles at 5 cm. as at 64 metres; such facts as these made it none too easy to detect the true state of affairs.

Other instances of the value of recognising the existence of this gradient will be better considered later, when analysing the lea test itself.

Thus far I have written of the readjustment process merely as a thing which happens, but it is also a thing which can be observed very easily. Except when very poor yarns are being tested it is not very noticeable in the usual half-metre test, but it is conspicuous if lengths of the order of eight metres are loaded

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The Intrinsic Strength of A at 240 grams was much higher than that of the control C at 185, but in the Lea test A was the weaker, giving 48 grams per strand as against 53 grams per strand for C.

Extrapolation of the gradient determined shows the Peak Strengths to have stood in similar ratio to those of the Intrinsic Strengths. Note that the deficit of the Peak below Intrinsic is greater than in Fig. 48, *e.g.* 240 grams drop to 140 grams; this is because similar cotton to that used in Fig. 48 is now being spun to a much finer count.

The Cross-over takes place at 1½ metres' length, the higher Peak Strength of A being there lost entirely by its steeper gradient. Tests which were made at this length only would infer that both yarns were identical.

Again, the Packing effect is greater with A than with C, so that an inferiority in the ratio of 36 : 29 at the Lea length is improved to 55 : 48 in the actual Lea test.

Whether A or C is the better yarn must evidently depend on the purpose for which it is used. Though the presumption is in favour of A, it is clear that no one test can discriminate between them.

and watched. The yarn is seen to rotate on its own axis, in one direction in one place, in the opposite direction in another; sometimes a length of only an inch or two will be seen to stand still, while neighbouring zones are spinning rapidly. An exact record of the movements of any one stretch of yarn would give a good deal of information with regard to the exact cause of its breakage; at present this can only be dealt with, by the next section, in general terms.

Local Strength and the Peak Strength

The next step is to consider the manner in which some very short length of yarn displays its strength; in other words, how do the weak links (which we have located when measuring the gradient) behave when they finally give way?

There are some features of this local strength which are mere special aspects of the problem, such as local weaknesses due to nip-ratching, shown in the chase-break curve (p. 98), weaknesses due to the mule-draw effect (p. 178), and weaknesses distributed pseudo-periodically over long lengths as the effect of drafting waves from the fly-frames (p. 128). These are better dealt with later, when the sampling of yarn for actual testing is under discussion (p. 221). Similarly there is no need to discuss the effects of humidity and temperature in varying the distribution of weak links, as well as in altering their weakness. Attention can be confined to a kind of normal.

It has been shown in Chaps. VII, IX and X that the normal state of affairs in any yarn includes a large and frequent variation in diameter due to the drafting waves generated in the machines. It has also been shown in Chap. XI that as the twist increases there appears a discontinuity in some yarn properties, as the adhesion of the hair ceases to be a limiting factor, and the actual tensile strength of the hairs themselves becomes predominant. Part of this tensile strength will be wasted in the resolved component of the stress which binds the hairs together, but this fraction is quite small (p. 176) on account of the very small angle of the twist; a full-twist yarn can be compared to a spiral staircase which rises through some thirteen times the width of its step (p. 171) in making one revolution round the central axis, and such a stairway would be much too precipitous for comfortable use.

Having regard to the enormous variations in yarn diameter it is evident that these themselves must be the principal determiners of local strength. The fact that the spinner's rule applies to these local variations has already been utilised to show (p. 171) that the effective diameter of the yarn is the same as its bodily diameter, so there is no need to consider concentrations of stress within any one cross-section in the normal case; no doubt such concentrations happen quite frequently on individual hairs, especially before the yarn has been conditioned, and their occurrence must vary locally as the yarn alters its form and structure under load; nevertheless they are incidental happenings, and in the main the stress is evenly distributed over the cross-section of the yarn at any point, leaving the variations of this cross-section predominant in the control of local strength.

The extent of these local variations of cross-section has already been illustrated by citation (p. 149) of the worst yarn produced in experiments on the effects of sorting. For my purpose here it will be more useful to quote the best of these yarns; this was No. 6, which has already been described (p. 157) as the strongest yarn of which I have any record for any cotton. The diameter variations of this yarn were the smallest in the series, but within a mere hundred measurements they gave $5\frac{1}{2}$ and 12 or more as the relative diameters of the thinnest and thickest places; again squaring these figures they show the relative difference in cross-sectional area, or equally in the number of hairs present, to have stood in the ratio of 30 to 144. The weakest link in the lengths of yarn measured was thus likely to be less than a quarter of the strength of the strongest link, or about half the strength of the commonest kind of link, in so far as the strength was determined either by the tensile strength of the hairs or by the hair-to-hair adhesion. There is no need to elaborate the point; evidently the variation of cross-section is quite enough to account for the occurrence of weak links, and so the important result is reached that, in the main, the Strength Gradient is a gross statistical consequence, and a measure, of the Drafting Waves.

Seeing that I had to admit inability to obtain exact measurements of the drafting waves themselves (p. 146), it is fortunate that this indirect measurement of their effects is practicable.

When the phenomena of readjustment are taken into account, the problems presented by local strength become necessarily

more intricate. The occurrence of breaks by slip and breaks by snap in the same singles yarn has formerly been cited (p. 184). The type of twist which is present at any given cross-section, whether "solid" or "flat," will probably have more effect on readjusted strength than on static strength. The extensibility of the cotton will also be concerned, because more extensible cotton will need to absorb more turns of readjusting twist into the over-stressed weak links, so as to maintain the action of the spinner's rule; this takes an appreciable time to effect by bodily revolution of the yarn (p. 212), and so brings in a bigger time factor; strips of rubber cut out with a varying cross-section, graduated, twisted, and loaded, serve usefully to illustrate some of these points.

None of these phenomena of local strength are easy to examine in detail. Corrections cannot well be applied to them, because there is no foreknowledge of the particular place at which the weak link will appear; corrections for variations of count and twist have to be used in gross tests, but it is barely practicable to determine count and twist except as the average of the length actually tested; this average can only be a rough kind of index to the count and twist at the actual point where the weak link pre-existed or developed. A general idea of the way in which local strength may alter in singles under the combined effects of tensional stress and twist readjustment can be obtained from sensitive stress-strain recorders, or even by careful watching of poor yarn under load. Local slips may happen in any part of the yarn, without leading to breakage at that part; sometimes they take place slowly, sometimes quite suddenly; as if, in the latter case, there had been an accidental concentration of stress on a single hair, which, breaking, allowed its neighbours to take up their share of the load.

The usefulness of doubling in stabilising the weak-link distribution is thus very evident. The same effect can be produced by other means, but without the same certainty and convenience; the singles yarn can be stretched in contact with roving, or just touching a strip of cloth. Either method will partly prevent the yarn from rotating on its own axis, but without the complete prevention obtained in the two-fold, and without the positive grip between the hairs which the two-fold provides in its core of compression (p. 187).

This takes me to another aspect of local strength which is of high importance in the interpretation of experimental data; it is conveniently described as the Clamp Effect. The existence of an appreciable torsional rigidity in yarn (p. 193) came under notice in connection with the "mule-draw effect" (p. 178); it recurs here as a factor preventing readjustment in singles for quite an appreciable distance from the clamp, or other anchorage of the yarn, at either end. If the clamps, knots, or cement at the ends are so arranged as not to damage the yarn, and the position of the place of break is then studied as it was studied in the chase-break method (p. 98), it is found that the distribution of breaks is not uniform all over the length. The departure from uniformity takes the form of few breaks in the middle, more at the ends, and this ostensible weakening of the ends by the clamps can be traced to a distance of several inches, even in fine yarn. This weakening near the clamp by prevention of readjustment in singles may be termed the Primary clamp effect.

There is also a Secondary clamp effect, which becomes predominant when the total length tested is only an inch or two. Such tests are practically worthless on account of it; examples of them will be found in the Thirteen Samples data, where it will be seen that they drop far below their proper place on the strength gradient. It may be objected by the reader that this shows the whole idea of the gradient to be erroneous, and this was also my own opinion for a long while. There can, however, be no doubt now, I think, that a much simpler explanation will suffice; the ends of the yarn are so close together that three or four hairs in contact can bridge across the whole interval between the clamps; further, the fixing of the ends puts unequal stresses on the individual hairs, and the mutual adjustment which takes place in free lengths of yarn is not so easy when the yarn diameter is an appreciable fraction of the total length. Both conditions lead to the development of local stresses, and the result of a strength test under such conditions is merely to measure the strength of some odd hairs. The observed fact that different samples show some consistency when tested in more than one such short length is not opposed to this reading of the result. Lastly, there is the fact in its favour that although a test at 5 cm. is approaching hair length, yet the strength is going down, and does not rise much nearer to the full total hair strength when the length is further

shortened to 1 cm.; in the last case almost every hair is gripped at either end, and the action of localised stress is self-evident.

Thus the ultimate difficulty confronting the student of local strength is this—that a fair test can only be obtained when the yarn is clamped by the yarn itself. A short length can be marked off in the middle of a half-metre or so, studied, and stressed till the yarn breaks; if, as is necessarily very probable, the yarn does not break in the length under observation, the process must be repeated. Thus, to obtain true information about the breakage of a 1 cm. region, the process would have to be repeated fifty times on the average in half-metre lengths to get one result.

Local strength is thus an imaginary quantity for the experimenter, though very real for the yarn. It would seem that the conception of readjustment could probably be extended much further than the gross readjustment of twist which has been considered; the "clamp effect" would seem to indicate that minute local readjustments of stress take place continually under load, these being made easy by the spiral packing of the hairs, which enables an over-stress in one direction to be resolved into other directions. The conception (p. 173) of the singles yarn as a self-adjusting structure is thus carried still further.

Peak Strength.—If the reader has now fully realised the imaginary nature of local strength, so far as any direct revelation of it can be obtained by actual experiments on short lengths, it will be easier for him to take the next step, for the Peak Strength of yarn is also an imaginary quantity.

The Peak Strength should signify the maximum local strength which the yarn can show, on the average, when the strength gradient is followed upwards to zero length. This, however, is an impossible definition, for the gradient is asymptotic, so that the Peak Strength would be infinite; at lengths equal to inter-atomic distances it would be the total of the atomic binding forces, and so on. The definition can be made practicable if the length at which the Peak is measured be taken at some reasonably small distance, preferably less than the length of a hair, such as one centimetre; this is admittedly arbitrary, but very convenient, because the same length was standardised for the testing of hair strength (p. 68).

Thus my definition of the Peak Strength is, the strength indicated by following up the strength gradient until this latter

intersects with the ordinate representing a yarn length of 1 cm. Being a local strength it is an imaginary quantity, and cannot be determined by direct experiment. Nor can it be determined with great precision, because the clamp effect prevents measurement of the strength in lengths which are shorter than half a metre, even in fine yarns, so that the gradient has to be extrapolated from this point through more than five subsequent halvings of the length in order to reach 1 cm.; if the shortest test were 64 cm., extrapolation would pass through 32, 16, 8, 4, and 2 cm. to reach 1 cm.

In spite of these disadvantages it will be found a very useful conception. It makes possible the direct comparison of hair strength with yarn strength which Dr. Bowman first tried to make (p. 200), though with misleading results. It is one of the two main components of the "theoretical strength of yarn," the other being the true weak-link gradient as shown in suitable two-folds. In the Thirteen Samples it will be seen that it may be identical with the Intrinsic Strength of the hairs, if the conditions of twist, hair surface, and consequent adhesion are such as allow their strength to be realised; or it may be less than the Intrinsic Strength if adhesion is a limiting factor, as it often becomes in singles yarn.

Thus the hair strength combined with hair weight can be used to compute the maximum Peak Strength which the yarn could possibly display, and the efficiency of the yarn-making process can be measured against this as a simple ratio of the actual Peak Strength to the possible Peak, or Intrinsic, Strength. It thus becomes possible to compare spinning efficiencies, so far as strength interests us, although very different types of cotton and yarn may be implicated in such comparison.

It will be appreciated, of course, that an absolute identity between Peak and Intrinsic is not possible, even after allowing for all experimental errors, but the differences will be small. A little tensile strength is wasted in the resolved compression (p. 172), acting at an angle of twist which averages about 16° when the surface twist angle is 25°. Care must also be taken to make the determination of the two strengths commensurable, by keeping the rates of loading similar in yarn and hair; failing exhaustive study of the effects which the Time Factor would produce with variation of loading rate, I evaded the difficulty from the com-

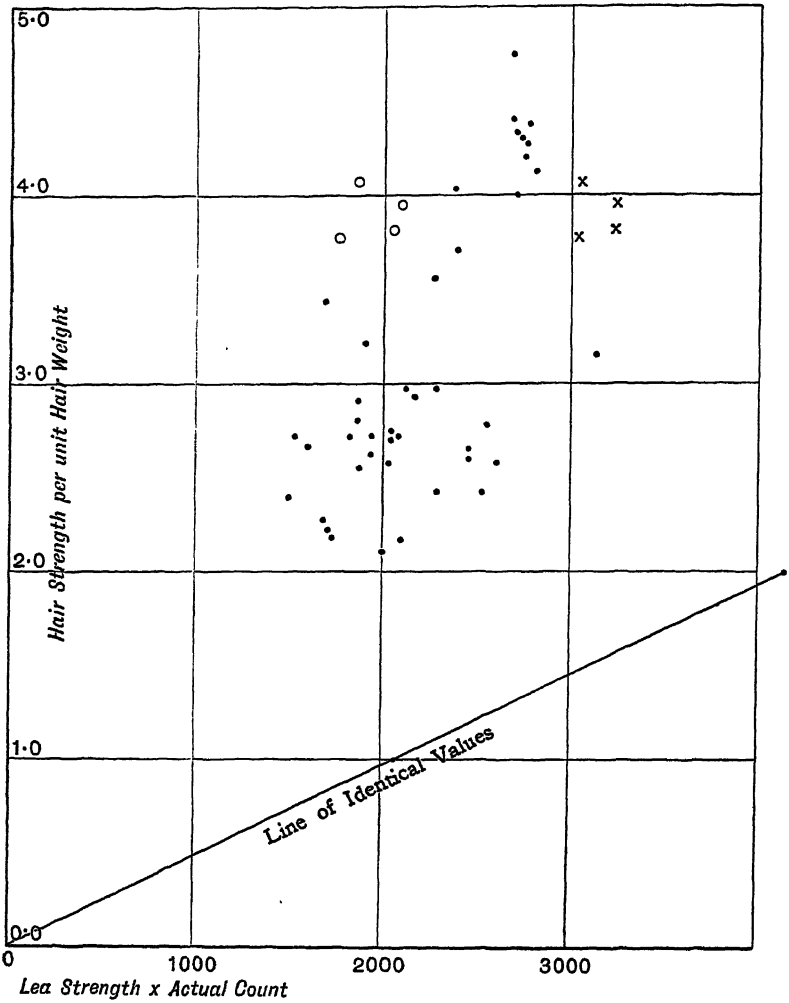

FIG. 50.—Showing the general relation between the Intrinsic Strength of yarn, and the " strength " indicated by it in the Lea test.

A wide range of yarns and cottons is included, each dot representing simultaneously these two values for any one yarn. The following counts and cottons are included :

Counts :—350s 240s 236s 180s 120s 100s 96s 90s 80s 75s 56s.
Cottons :—St. Vincents, Montserrat, Ceylon-grown Sea Island.
Sakel, Maarad, Casdagli, Casuli.
Zagora, Pilion, Uppers.
Long Staple, Durango.
Tanganyikas, Ceylons.

The " line of identical values " is that on which all the dots would fall if the

mencement of my investigations by adopting rates of loading which are broadly similar in both cases when expressed as the time taken to reach the average breaking load. Critical studies would probably need exact adjustment of this rate, which is not an easy matter.

One of my colleagues kept a record showing the correlation between the composite strength observed in the lea test (p. 233) and the Intrinsic Strength of the hairs for a very mixed bag of full twist yarns, ranging from 25s to 250s, and from Indian to Sea Island. The existence of a general relationship was clearly indicated, but the relative yarn strength tended to fall off as the absolute Intrinsic Strength increased; this was due to the fact that the cottons with a high Intrinsic Strength were mainly long and fine ones, as already noted, and so had been spun into fine yarns. Thus, this falling-off in the relationship means that fine yarns are less efficient than coarse ones; this we have seen other reasons to expect, in the relation between drafting waves and roving thickness (p. 128), and in the larger percentage of surface hairs (p. 184). It was tested by spinning some of these fine cottons into much coarser counts, when the strength moved up very much nearer to the Intrinsic Strength. Though detailed study of Peak and Gradient was not possible in every case, there can be no doubt that both the Peak Strength of the singles and

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two quantities could be identical, which is impossible on account of the existence of the Strength Gradient between the 1 cm. length of the hair test and the 120 yards length of the Lea test. This line is calculated from the following considerations:

The "unit hair weight" is standardised at 0·001 mg. per cm.; this is equivalent to a count of 5900s. The hair strength of the raw cotton of each sample plotted is computed as fractions of a pound, and multiplied by this count of 5900. The product is then further multiplied by 160, which is the number of separate strands loaded in the Lea test. The result is directly comparable with the Count × Strength Product in which the results of the Lea tests are expressed. Differences in count are thus eliminated from the comparison in this diagram.

The calculation reduces itself to the simple result that if the Intrinsic Strength of the hair (as expressed in grams per cm. of a standard hair weighing 0·001 mg. per cm.) is multiplied by the figure 2080, the result is the equivalent Lea Strength (expressed as the count-strength product). This, without any allowance for the reality of the gradient, or for losses of strength due to imperfect adhesion.

Out of the fifty samples shown, the weakest one gives a Lea result which is 25% of its Intrinsic Strength, and the strongest reaches 55%. Though these measurements of the "efficiency" of the spinning process are meaningless in themselves, they should be compared with the 10% value obtained by Bowman (*v. s.*).

Four cottons are shown twice over as circles and crosses. They are superfine cottons spun first to 250s (circles) and then to 80s (crosses). The loss of "efficiency" in the former case is very marked.

the Gradient also were improved by thus using coarser counts; the Intrinsic Strength is, of course, unaffected. These unanalysed data are interesting through the indication they provide concerning the relative efficiency of fine and coarse spinning. The former seems to suffer much more from excessive gradient, due to the difficulties of drafting. In view of the "10% efficiency" shown by Bowman's rough estimates (p. 201), it is of interest to observe that the minimum "efficiency" in this varied record for full-twist yarns was 25%, while the maximum was 55%.

Yarn Testing

It is evident from the foregoing discussion that it is possible, but not easy, to arrive at an analysed definition of the strength of any yarn. It should also be evident that some method of testing which would make this analysis easy would be well worth having; such a method is still to seek, and in the present Section I can only describe such features of the present methods as are relevant to the preceding Sections, leading up to a study of the meaning and limitations of the convenient standard lea test.

It has been seen that real yarn, even the very best real yarns, are very far removed from the "perfectly cylindrical yarn" which is sometimes mentioned. The perfect yarn would break up all over at once, I suppose, like Oliver Wendell Holmes' "one-hoss shay." On the other hand, we have seen that the intrinsic peculiarities of real singles yarn are such that, in spite of its imperfections, it does its best to emulate that vehicle. Consequently there is some justification for the casual way in which the testing-room of an ordinary mill is employed; four lea tests, even two, are made in order to measure the "strength of yarn" from a sample, though days may have been spent in conducting it through a spinning test. At the other end there has been some justification for multiplying the number of test repetitions far beyond the limits of practicability, treating the problem of yarn strength as if it were insoluble, and piling up into the curve of error thus obtained such dissimilar factors as drafting and hair strength, count variation and slack bands, nip-ratching and rough rings. It is therefore advisable to examine some features of sampling, and of the correction of observations, before considering the available tests.

Sampling.—By far the most important variable which must be dealt with in sampling is the variable "count." When long lengths are tested, as in the lea test, it is separately measured and used to apply a correction. To measure the count of every strand tested in single thread is not so easy, and is rarely practised (p. 368). Consequently, such variations of count as may be found in short-length tests are usually dealt with by sampling; the count of the total length to be sampled is ascertained, samples are then taken from the whole of this length, and an average correction applied to the average result.

This method is insufficiently exact when the strength gradient has to be determined by the testing of various lengths in single thread. To deal with this it is advisable to recall an early experiment formerly quoted (p. 131), in which a wave-like grouping of weak and strong places was observed. These were obtained on an automatic single-thread tester which was set to test some thousands of consecutive lengths, all alike, along a single cop of yarn. Having been observed, they were traced to similar variations of count, which have been discussed as "drafting waves" (Chap. VIII). Knowing that such is the type of count variation found in yarn, it becomes evident that the most dependable way of obtaining comparable counts for testing at different lengths, for gradient, is to take those lengths one after the other repeatedly, from each cop tested. Here it is not directly important to know the average count, so long as that average is the same for each of the lengths tested. Thus the lengths to be tested for gradient determination must at least be drawn as 8, 4, 2—8, 4, 2—8, etc. It is debatable whether a sounder plan would not be to draw the maximum length every time, and to remove the particular length to be tested from the middle of it, still following the same order. The simpler method is the one which I have employed.

Another peculiarity of yarn which is in essence also a count variation is found in nip-ratches (p. 94). These can be disregarded altogether on the assumption, which I have allowed to apply throughout this chapter, that their strength properties follow those of count variations due to actual drafting. Or, they could be eliminated for more exact work, by discarding the first break, and re-testing the residual halves. Or, their relation with the mule draw having first been followed up by means of a B.T.P.

diagram (p. 93), the sampling could be related to the length of the draw or chase, so as largely to avoid them.

In the same way another important variable may sometimes require consideration in the graduated distribution of twist over the chase by the mule-draw effect (p. 178). Whether it may be advisable or not to make the length of each chase the unit length for testing will depend on the feature of strength which is being investigated. Evidently there is a decided advantage in testing at chase-length, when a direct comparison of single-thread strength at some arbitrary length is all that is needed; both the mule-draw effect and nip-ratches are thus automatically eliminated from the contributory variables to be sampled.

The last important variable of twist is peculiar to individual spindles. A band which is slightly too slack or too tight will vary the average twist, and it has been shown that even a slight modification of twist will produce a big alteration of strength on the steep part of the twist-yield curve (p. 175). With ring spindles there are liable also to be variations in tension from one traveller to another, due to minute roughnesses on the ring itself (p. 103); this alters the tension, and so may modify the completeness of hair-to-hair adhesion, thus slightly affecting the strength. These errors of individual spindles can sometimes be dealt with by random sampling of a large number, but when the practicable limit of number is only four, as in spinning tests, individual attention must be given to each spindle, and to the twist of the yarn produced upon it. When only a single lea test is taken from each spindle, as in most mill testing-rooms, this error is likely to escape detection.

Corrections.—The application of corrections to the bald experimental values obtained for strength is highly desirable. They can be applied by measuring such concurrently variable determinants of the strength as the count, or the twist, ascertaining the statistical inter-relationship, and shifting the experimental value up or down accordingly. But unfortunately this is not too easy. There are two main difficulties.

The first difficulty depends on the fact that the strength measured by the experiment is a local strength, whereas the exact count or twist at the locality which breaks is necessarily almost unmeasurable (p. 190), and must be inferred as an approximation from the average count or twist. Here again the conception of

the strength gradient is serviceable; being in itself a statement of the correlation between length and strength, it measures indirectly the relative sizes of weak links, though not their absolute dimensions.

The second difficulty is that the closeness of such correlations varies from one yarn to another, and the ratio also. This can be illustrated by reference to the plotting (Chap. XIII) of count-strength correlation diagrams for the Thirteen Samples in their lea tests; the concentration of the dots will be seen to differ from sample to sample, as also does the slope of a line drawn through the most likely long axis of the elliptical group. Now the determination of correlation coefficients is none too easy with such variable material as yarn, and it will be obvious from inspection of such diagrams that the more uniform the yarn, the less easy it is to place the line which describes the correlation coefficient in its correct angle, or to estimate whether it is really a hard sharp line or a vague smudge. Consequently there is some advantage in spinning yarn which is deliberately made variable, so as to make this determination of correlations easier; it is in this direction that there is some hope of simplifying the equipment at present needed for spinning tests (Chap. XV).

In spite of these difficulties the attempt at correction of data is well worth making, not only for practical ends, but also because most interesting information sometimes emerges from this study of inter-relationships. Some of it is so unexpected as to be baffling to the student for a time, until eventually it fits into its place in a more complete hypothesis. Though the main arguments of this sub-section apply to every property of yarn which can be measured, yet it will be simpler to confine discussion to the count-strength correlation as being the most important one. Before going further, the statistical significance of such correction procedure must be defined, and its consequent practical importance.

The high variability of yarn strength has already been mentioned, as also the statistical fact that the significance of the average result obtained from a number of tests is inversely proportionate to the square root of that number (p. 205). Since an increase in the number of tests is thus beneficial, it is very well worth while to double the accuracy by making four tests instead of one, sixteen instead of four, or sixty-four instead of

sixteen. At the start we double the accuracy by making three additional tests, then double it again by making twelve more, and again redouble it by another forty-eight; the next doubling would require a hundred and ninety-two additional tests. Evidently there is a stage at which considerations of accuracy are balanced by those of patience and cost; a hundred tests is a convenient limit. So long as only a single measurement is taken in each test we can no more escape from this square-root law than from the force of gravitation.

Another form of the law can be brought into service, however, if any other property which can be measured in parallel with strength has any relation thereto. Taking the Count as being such a property, the degree of connection between count and strength in our data is first computed, thus expressing this connection as the Correlation Co-efficient, denoted as r. This value of r may range from 1·0 when the correlation is perfect, down to zero when there is no correlation whatever. In the latter case it would obviously make no difference to the significance of the strength-test results whether the count were brought into the story or not. Conversely, if the correlation were perfect, the variability of the strength tests would completely disappear on making the count correction which it indicated; nominally, a single test would then give a perfectly true answer.

These two extreme cases, as well as all intermediate ones, are covered by the expression $\sigma\sqrt{1 - r^2}$.[1] Thus, if the correlation of count with strength worked out at $r = 0{\cdot}7$, our estimation of strength in terms of count would be subject to a variability, which may be called the "probable error per cent.," based on this as well as on the probable error per cent. of the single isolated strength test. For this latter I have already cited 10% as an exceptionally low value, and 15% is more usual. The value for strength after correction for count thus works out in these circumstances at a P.E. % or 10·7, this being equal to $15\sqrt{1{\cdot}00 - 0{\cdot}49}$. Applying the square root law to this it follows that the average of fifty-one tests would have a probable error of 1·5%; this is the same error as that of a hundred tests made baldly on the strength alone with the 15% error on each.

Thus the simultaneous measurement of two things which are fairly well correlated has halved the number of tests needed to

[1] Yule, G. Udny, *Theory of Statistics*, London, 1924.

attain a given standard of accuracy. If an accuracy of 0·3 P.E. % were desired, the saving would amount to more than a thousand tests, which is a conclusion of some practical importance.

I have stated that the exact degree of correlation is not always easy to find. In the routine of mill testing-rooms a count correction is always made upon the lea-test results, bringing them to "strength at nominal count" by various formulæ with rational bases. The simplest of these is in many ways the most convenient; it consists in merely ascertaining the product of Strength × Count. This is essentially an assumption that the correlation of the two is perfect, because count is roughly the reciprocal of the cross-sectional area in a constant length, so that the Count × Strength product is a rational quantity, and should be constant for the same cotton in all counts *if* other things are equal. Different kinds of cotton may thus be compared in all counts, and no big effort of memory is needed, since one has only to remember that a value of 3,000 is excellent, 2,500 good, and 1,500 bad, for full-twist yarns. The following table may be quoted from a text-book [1] in illustration, after conversion of some of the lea strengths there given into this form. Detailed consideration is unnecessary.

	English Warp Yarns. Ordinary Cottons.			*Egyptian or American.*			*American* (Ring).
	Common.	Fair.	Super.	Common.	Fair.	Super.	
10s	—	—	—	—	—	—	1687
20s	1500	1700	1960	—	—	—	1760
40s	1440	1600	1760	2000	2200	2400 [2]	1784
80s	—	—	—	1600	1760	2000	1968

But in spite of its convenience, this generalised measure can completely conceal information if used blindly, for the two things are not always correlated simply. In a yarn of moderate twist the correlation will be typically inverse; such that the higher counts are the weaker in consequence of their smaller cross-section. On the other hand, a heavily over-twisted crêpe yarn

[1] Thornley, T., *Advanced Cotton Spinning*, London, 1923.
[2] The figure for No. 6 sorted yarn (p. 147) was 3200.

gives a correlation which is exactly inverted, or direct, because the yarn is weakened by over-twisting, and this will be more severe, as expressed in the form of twist-angle, in yarns of bigger radius and hence of lower count. Half-way between these two the correlation is zero, in the transition stage, and nothing is gained by including count in the calculation of strength. The strength of the crêpe yarn would be better expressed in terms of the count reciprocal, as a "Weight × Strength" product.

The ground is safer when the Count × Strength product is used only for comparing yarns of similar twist, as I have largely used it, but even so it is an approximation. Such evidence as that quoted and unquoted for the Thirteen Samples shows that we can find such changes in the correlation amongst yarns of similar type as make it difficult to evaluate fine shades of strength. It is fortunate that my present thesis has only to make first approximations, sufficient for analysis of the Thirteen Samples, which range between lea strengths of sixteen and twenty pounds.

The application of similar corrections to the single-thread test is not usually practised, though they are more important here than in the relatively complex lea test. It is by no means impracticable to do so, if a simple device is attached to the tester (p. 368) to mark off a fixed length at constant tension, and a torsion micro-balance is used for quick weighing. The effect of omitting such corrections can be realised by considering what would be the result of uncorrected testing upon the few leas of yarn already cited (p. 131) as an example of the pseudo-periodic variation of strength.

The correlation of count with strength is still further complicated in single-thread testing for gradient by my alterations of the length tested. It has been shown that the conditions under which single yarn is stressed in short lengths are much distorted by the clamp effect (p. 215). Therefore it should not be altogether surprising now to meet with the extraordinary result shown in the next chapter, where the correlation coefficients in a series of samples turn over completely in passing from lea tests to single-thread tests made at 6 cm., until they assume exactly the inverse relative order of that which they first presented. Where there had been a high count-strength correlation in the lea, there was a low one at 6 cm.; where it was low in the lea, it became high in the clamp-affected short length. This was another of the minor

paradoxes which led me to distrust all yarn tests for several years. Yet if the conception of Readjustment is followed up, the paradox vanishes; very irregular yarn readjusts freely, and in long lengths can only show small variations in its average count, hence its count is a minor factor in long-length tests, and so gives a low correlation; whereas, being thus irregular, it is very variable in its short-length counts, and this irregularity is directly reflected in the test result, because the clamp prevents readjustment, and so a high correlation ensues. The converse applies to regular yarn.

It should be clear that the correction of strength results, although very desirable, should only be undertaken with the utmost caution, and should be based upon direct measurements of the correction factor, made at the same time, with the same yarn, under the same test conditions. In course of time our knowledge will be wide enough to predict what these corrections should be; for the present it is advisable to go very carefully, treating each yarn tested as a special case.

Lastly, there are some subsidiary devices which can hardly be dignified by the title of "corrections," being rather of the nature of eliminations or "rejections." They are useful when used cautiously. The best example is provided by concurrent measurements of the strength, count, and total extension in single-thread tests. In so far as each strand of yarn tested goes through the same changes under load as do its fellows, so these three should all be correlated. Three correlation diagrams can be plotted for strength count, strength extension, and for count extension. These are then used as "target diagrams"; [1] any observation which lies very far outside the main group on some one target, or not so far out but showing up in more than one target, may be struck off the list of dependable observations in computing the average strength.

It remains to examine the actual testing process.

Tests by Reciprocating Stress.—From the foregoing discussion it would seem that tests in which the loading oscillates will be rather complex. Results will vary with the method by which the load is removed on each cycle; it has been shown that a sudden release from tension causes negative permanent set (p. 191), the yarn structure opening and shutting like a spiral concertina.

[1] W. L. B., *Raw Cotton*, p. 134.

Increase of strength through readjustment (p. 172) will be exaggerated in so far as the Time Factor has more play in these prolonged exposures, and also it will be facilitated by vibration. On the other hand, the alterations of tension will also act in the opposite direction eventually, breaking down the hair-to-hair adhesion (p. 184), whereof negative permanent set is merely an extreme case.

While reciprocating tests have practical usefulness, they do not at present help much in the analysis of yarn strength, though they should serve a useful purpose in estimating durability. Thus it is probable that the short-hair yarns described in Chap. IX might show more inferiority in such tests than they do when compared with the long-hair yarns in dead-load tests.

A limit case of such tests provides quite the prettiest experiment which can be made with yarn. It consists in watching a suitable length of singles yarn put under some small tension with one end fastened to a vibrating reed or tuning-fork and the other end fixed; the reed is kept in vibration at audio-frequencies, through an amplitude which is less than 1% of the yarn length. The vibration is arranged to take place fore-and-aft along the yarn, like the major vibration on the mule spindle (p. 177); the tension, length, and count of yarn are so adjusted as to make the vibration begin with the fundamental, one long loop from end to end of the yarn. Although the initial stress is small, and the strain also small, the yarn begins to fail very quickly, and the vibration figure changes accordingly. Thus the original single loop breaks up into all the succession of changes which result from the fall of tension as the yarn extends. The vibration being stopped when some eight nodes and loops have formed, the yarn is seen to hang in a slack loop, which can be pulled apart by slip at some part of the length, like a sliver.

Testing by Impact.—An inexpensive and accurate type of pendulum tester which I designed for this purpose is described in the Appendix (p. 355). The impact test measures the energy absorbed in the act of breaking the yarn, and it has great advantages of convenience. Yet it has been repeatedly advocated and discarded by students of textiles, and even with metals there is some difficulty in its interpretation by the engineer. Some reasons for this will be worth comment.

This test eliminates the Time Factor; therefore it is not

necessary to consider yarn readjustment. Hence it follows that it does not really test the more usual behaviour of singles, and their most special characteristic; but this is really an advantage in using it to foretell the properties of finished goods. So far it might be expected to give results like those from two-folds under dead load.

The fact that it gives the true average resistance to impact, however many strands may be broken simultaneously, is its greatest advantage, and is an emphatic contrast to dead-load testing.

Its fundamental weakness consists in the dependence of its indications upon two, or more, simultaneous variables. There is the breaking load or resistance proper upon the one hand, and the extensibility on the other; the resistance to impact should agree with the formula "$\frac{1}{2}Re$." But both these variables are themselves composite in yarn; hair strength and hair slipperiness being involved in the one; elasticity and permanent set, hair properties and constructional properties in the other. Simply as a general principle, it is well to be chary of importing additional variables into a testing method.

It is barely practicable to analyse the impact results by making independent determinations either of "R" or of "e"; the Time Factor intervenes, so that dead-load measurements have no exact relation to the reality of instant breakage. Possibly a simultaneous record of the actual "e" might be made during the impact blow, so that "R" could be obtained by simple division. But I am dubious whether, even so, the values obtained for "R" would be real ones.

My doubt here is one which I have not been able either to verify or to abolish; it is based on the fact that, in such a structure as yarn, when extending and then disintegrating under a sudden blow, the friction of hair on hair absorbs a large amount of energy; this energy would seem to be additional to that which is required to overcome the elasticity of the hairs, or of the yarn as a whole, or the energy of the actual breaking resistance. In most materials, such as metals, this is a small quantity, in yarn it is a large one.

The ambiguity of a bald impact test is clearly demonstrable from the Thirteen Samples data by applying the same device as before, in making comparisons between tests at various lengths (p. 258). Some further analysis of the meaning of the impact

result is also made possible thereby. It will there be seen that the form of the distribution between the various samples is appreciably altered by changing the length under test from 21 cm. to 5 cm. This cannot be ascribed to the primary clamp effect, for readjustment has no time in which to act; nor can the secondary clamp effect be operative, because the results of impacting are not dependent on simultaneity in the rupture of all the hairs. A reasonable cause for this alteration is indicated, however, when the distributions at these two lengths are compared with the distributions of " total extension in yarn " and with " hair slipperiness " respectively. The 21 cm. impact has a resemblance to the former, the 5 cm. impact to the latter; a 12 cm. test which is not figured was intermediate in form.

Thus there is a kind of " fade-out " in the impact results as the length under test is changed. The strength represented by " R " in the formula $\frac{1}{2}Re$ will increase only slightly in passing up the gradient 21 cm. and 5 cm.; the halving-change in these samples lies between 4% and 8%, and there are only two halvings, 20, 10, 5. On the other hand, the total amount of the extension " *e* " will decrease almost directly as the length, though not exactly so. Thus in a typical case the value of " R " would rise from 100 grams only to 113 grams, whereas that of " *e* " would fall from 110 to 30 hundredths of a centimetre, which is an enormous alteration relatively. This accounts for the fading away of the resemblance to yarn extension.

It remains to consider why some sort of resemblance to hair slipperiness should take its place, probably upon a substratum composed of the hair strength. This seems to suggest that even after the hairs have been broken at the weak link, as well as before, energy has to be expended in dragging them over one another (Chap. XV). But whether 21 cm. or 5 cm. be the length tested, the length of the zone where the local strength breaks down will be substantially unchanged. And the size of this zone, taken relatively to the total length tested, is four times as big in the 5-cm. test.

So it would seem that the impact test must be dominated by the length in which the test is made. If the length were made infinitely long it would simply record the total extension alone! At infinitely short lengths, even perhaps at 1 cm., it would measure only the intrinsic strength. Anywhere in between these

limits it measures intrinsic strength, strength gradient, extensibility, and the hair slipperiness or adhesion or draftability, all together and not easily separable. But it measures them in the absence of a time factor.

Yet it would not standardise the test to standardise the length in which it is made, because the percentage extension of yarn is itself a variable. Multiple length testing seems the only practicable way in which it can be made of use, using the results as sets of simultaneous equations, containing not less than three unknowns.

Testing Single-thread by Dead Load.—Recognising that this form of yarn test is the basic one, in terms of which all other tests must be interpreted, my earliest investigations were directed to comparing the various existing test methods, and to developments of them. The test has certain outstanding defects: first, the high probable error, which is commonly $1\frac{1}{2}\%$ on the average of a hundred breaks, so that the significance of such an average lies only between limits of 95 and 105; secondly, the time and labour consumed in making so many tests; thirdly, the inability of existing machines to test in varied lengths outside the range of the clamp effect; fourthly, insensitivity of the loading indicator. Taking these four difficulties in order:

The high probable error is inevitable, being the direct consequence of the yarn structure. Further particulars will be given in the next sub-section of this chapter, and some means whereby the observed facts may be partially corrected have already been described (p. 226).

To reduce the labour in testing it is feasible to use automatic devices, but saving of time can only be effected by eliminating waste of time between successive tests, since the loading rate cannot be unduly hastened without bringing the test conditions too near to those of impact. This consideration handicaps the very beautiful Moscrop machine. An automatic dead-load pendulum tester (p. 348) was therefore devised for use in place of the Moscrop.

As the importance of testing in many lengths became evident to me, this automatic instrument was found nearly as inconvenient as the usual types, and the novel arrangement of the Yarn Dynamometer replaced it (p. 367). Lengths ranging from 64 metres downwards have been studied on this appliance, and

the usual changes (p. 182) between 8, 4, 2, 1 and ½ metre are effected with it as routine procedure after each test; but it is not automatic.

The fourth point was examined by sending the same yarns to several testing-rooms in various types of institution. Comparison of the resulting data was most unsatisfactory. Even tests made on the same make of machine were discordant with fine yarns, the true frequency curve breaking up into clumps around certain values, thus clearly indicating abnormal resistances in some values of the loading. The design of the pivot and the ratchet-lock was evidently unsatisfactory in prolonged use. Since then Dr. Shorter [1] has located the inertia error definitely as another fundamental defect of most testing machines.

With this passage of time, and the further development of these studies, it seems quite certain that the type of single-thread tester which will fulfil all requirements is one which I have so far failed to design in any reasonably simple form. It should test on spring-loading, with a simple accessory for routine checking of the zero and calibration. It should test from 8 metres to ½ metre or thereabouts, in succession along the same cop; either quite automatically or, as in the Hair-break tester (p. 355), from a magazine previously loaded. In either case the record must be graphic, and kept in serial order. The rate of loading should be maintained approximately constant at all lengths; this involves the provision of an adjustment whereby to compensate for the different extensibilities of different yarns, as well as a constructional compensation for the different total extensions of the five different lengths. As such an instrument must occupy a good deal of space on wall or bench, it should be quickly convertible to three different calibrations, like the Yarn Dynamometer, so as to handle fine singles or coarse cords with equal relative accuracy. It should not aim at great accuracy in the measurement of individual breaks, so long as the observational error was symmetrical. Simultaneous records of the total extension are desirable, so recorded that they could be read in pairs with their equivalent breaks. Lastly, it is even more desirable that the broken ends should be preserved in their proper order, to be subsequently weighed for count.

Such an instrument would make single-thread testing a

[1] Shorter, S. A., *Jour. Text. Inst.*, 1923.

source of invaluable information in the mill. Some twenty-five breaks at each length would suffice, and the whole test could be completed and corrected in half an hour. Since permanent reliability and "fool-proofness" are very important, it would probably cost more than all the rest of the simple equipment of the usual mill testing-room put together. It is rather a possibility than a likelihood at present.

A description is given in the Appendix (p. 348) of the apparatus with which I have clumsily obtained the same data which this machine would easily provide. Tests of very short lengths can also be made on the Dynamometer, but most of those quoted in Chap. XIII were carried out on a Hair-break tester; in order to ensure greater exactitude of comparison with hair strength, the standard instrument was fitted with a proportionately heavier pendulum for this purpose, and lengths of 10, 5, 2½ and 1 cm. can thus be broken.

The results obtained in this way are interwoven throughout this chapter. It remains to consider their employment in the interpretation of the lea test.

The Lea Test and its Meaning.—It is by this test that the strength of yarn is assessed in everyday practice. The rapidity with which it is made, and the consistency of its results, confer advantages upon which it is hard to improve. On the other hand, judging it as a physical experiment performed on a skein of fine wire, hardly any method of testing could be more vague and ill-defined.

By making use of the preceding pages the reader will now find it a comparatively simple matter to analyse the lea test into its component parts, and so to attach a precise meaning to any measurement obtained thereby. It may be noted as a matter of experimental interest that most of this analysis of "yarn strength" as such was effected during the process of dissecting the lea test, so that the present account is written mainly in the reverse direction from that in which the experiments were made. However, apart from the convenience of explanation, there is a further appropriateness in so doing, because it allows an outline which began in the everyday practice of the Grader to be completed at the everyday test applied finally by the Spinner.

The original experimental study of the lea test was made on

two very good yarns of dissimilar type, an aeroplane twist and a soft weft, in the course of training a junior colleague to research.

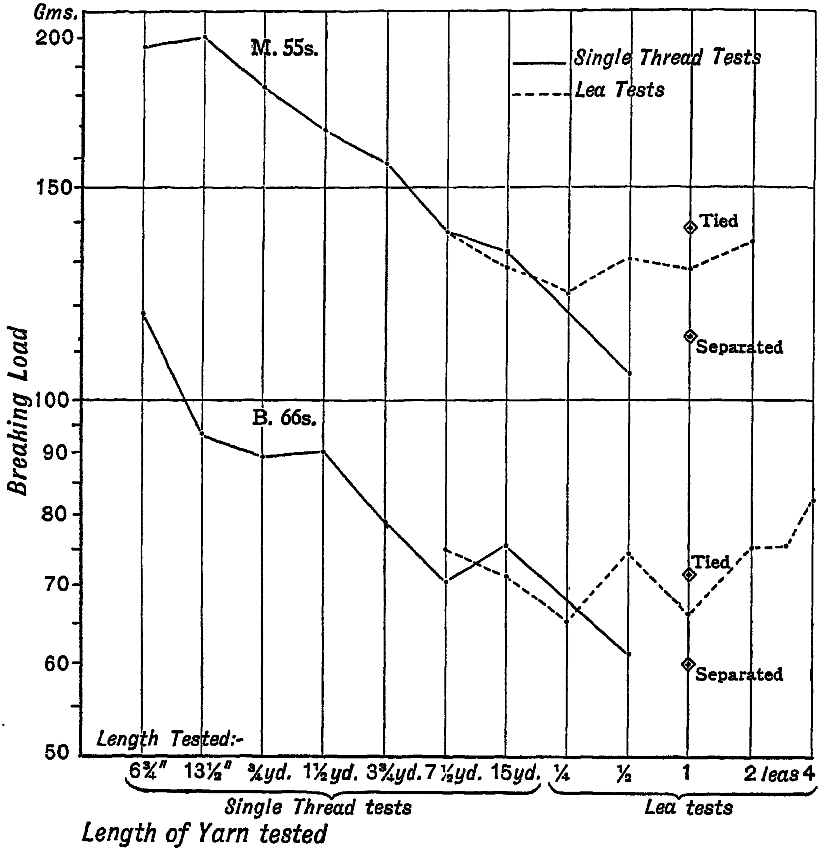


FIG. 51.—Gradient and False gradient in Lea tests. Demonstrated on twist and weft yarns. The continuous line represents the single-thread tests, the dotted line the tests made by the Lea tester, in the lengths indicated from 480 yards downwards through twelve halving-changes.

Two points bracketing each curve on the one-Lea ordinate show the effects respectively of increasing the Packing effect by tying the strands of the Lea into a compact bundle, and of decreasing it by keeping them all separated. In the latter case the yarn was wrapped on a paper foundation, which was cut after being transferred to the tester.

The properties of these two yarns were measured in single thread until the components were isolated, which the reader now knows as the Strength Gradient and the Peak Strength. On following

the gradient down to the lea length of 120 yards it was found to give a value somewhat smaller than that obtained with similar loading rates in the lea test itself. A rational basis for the lea test was thus indicated. It remains to examine the cause underlying this comparatively small disagreement.

For this purpose the lea was wound upon wrap-reels of varying diameter, thus altering the number of separate strands which carried the test load; normally this number is 160 strands, each 27″ long.[1] The test was also made upon multiples and fractions of the usual single lea. The rates of loading were varied. Normal tests were compared with others in which the skein was so arranged on the hooks of the tester that each individual strand was free from contact with its neighbours; this was extended further by substituting a sheaf of pulleys for the usual hook, so that each strand could balance its tension more freely against that of the others. Count corrections were made by the correlation method to avoid false assumptions. Humidity errors were largely eliminated by interlacing the times at which various experiments were made. The numbers of tests employed were balanced between the probable error, as ascertained in every case, and the resources for experiment then available.

I have said that the lea test gives a rather stronger break than the same length tested in single thread. These two values are, of course, incommensurable unless the lea strength is corrected to the loading of single strands; this is done when the break shown on the lea tester is divided by 160, the number of strands between the two hooks. When this number is sufficiently small, as when testing one-eighth lea, the lea-tester strength is identical with that of the same 15-yard length in single thread. This gives an intercommunication between the two methods.

When this number of strands is further increased through the quarter-lea and half-lea to the usual lea test, the lea-tester results rise progressively further above the true Strength Gradient. This rise roughly balances the true gradient's fall by the time that the single lea length is reached, so that small deviations above or below lea length have no effect in this region on the indicated strength per strand. This is a minor good feature of the lea test. Further increase to multiples such as two, three, and four leas makes this "False gradient" shown by the lea tester

[1] *Vide* p. 201.

turn upwards, more and more rapidly. Evidently some new factor, quite independent of the true single-thread strength, is becoming increasingly important.

This new factor may be called the "Packing effect," for it is eliminated if the strands are not in mutual contact. It is not due to readjustment of twist along the strands being partially prevented where they touch one another, for it has been shown already that this prevention would have a weakening effect, not a strengthening one. Nor is it due to a clamp effect extending from each end of the skein, where the strands clamp one another as they pass over the hooks of the tester, since this also is a weakening factor, though only a small one in 27″ lengths. The Packing effect outweighs both these, and can only be due to the mutual support which contiguous strands give to one another at, and near, the hooks; local stresses are thus shared among them by yarn-to-yarn adhesion under the lateral pressure, which varies from a maximum on the hook itself to a minimum midway between the two hooks. When the hooks are stuffed full with multiple-lea lengths, this resolved lateral pressure is quite appreciable, even in the centre of the skein; the skein can be seen to be fatter there than at the hooks. In reality, then, the Packing effect amounts simply to cutting out an appreciable length of yarn from submission to the testing stress, taking this in short lengths from the ends of every strand. Observation of the break-place in the lea test substantiates this, for the breaks tend to concentrate in the central part. The Packing effect is in some ways a good feature of the lea test, for it counterbalances some of the "artificial" properties which readjustment confers on singles yarn; it is in such a "packed" condition that the yarn is ultimately used in cloth.

Although the lea result depends primarily on the strength of the weakest place in the lea length, it is at first perplexing to find that the two ends of the skein can be left free without seriously affecting the result; these free ends represent an infinitely weak place. Nor is it true that when the first strand breaks down, all the others follow suit; careful observations on the breaking-time show that, more often than not, one or two more strands have to give way before the load will cease to rise, and some more before it can fall, if left free to do so. These anomalies can be traced to the mutual clamping of strands on the hooks, and it is

largely due to them that a fixed loading rate is desirable; actually, the present standard machines involve the yarn extensibility in this rate. Similarly, the Packing effect calls for care on the operator's part in placing the lea quite fairly on the hooks. That the result of a lea test can be modified for buying, and for selling, is a stock jest.

The variability of the result is noticeably less than that of tests in single thread, but this advantage is fictitious. It seems to be due to a kind of masking of the real variability of the yarn underneath the Packing effect, for the variability decreases as the Packing effect increases. Thus the range of difference really existing between weak and strong singles yarn is still further disguised.

The magnitude of the Packing effect itself varies from yarn to yarn, seeming to follow the hair slipperiness; or, rather, the slipperiness of the yarn, to which the hair slipperiness is partly indicative.

Some speculation upon the further development of the lea tester may be permissible. If its advantages can be utilised to give any indication of the relative parts played by gradient, local strength, and Packing effect, without sacrificing too much of its handiness, its improvement is worth seeking. But again, as with single-thread testing, demand must precede supply, for an improved lea tester would be more costly. Its sensitivity should be alterable, quickly and safely, to test fractional leas. Its wrap-reel should be able to lay out skeins in which the strands were either separated or in contact at will, and the reel itself should be transferable to the tester, so as to avoid disturbing their arrangement; this calls for re-design of the hooks also. The single-thread gradient could be thus obtained, and the Packing effect by difference. It should employ a fixed and definable rate of loading in place of the present variable rate which is unavoidable in the present designs; a miniature of the engineers' testing machines would seem to be indicated, and this would avoid the inertia error, while permitting the use of flexible metal links in place of the present pivots. The lea tester thus re-designed should be able to trace the stress-strain curve with sufficient delicacy to enable the breaks of the first few strands to be located individually; some new American forms of the machine are an advance in this respect, but do not go far enough.

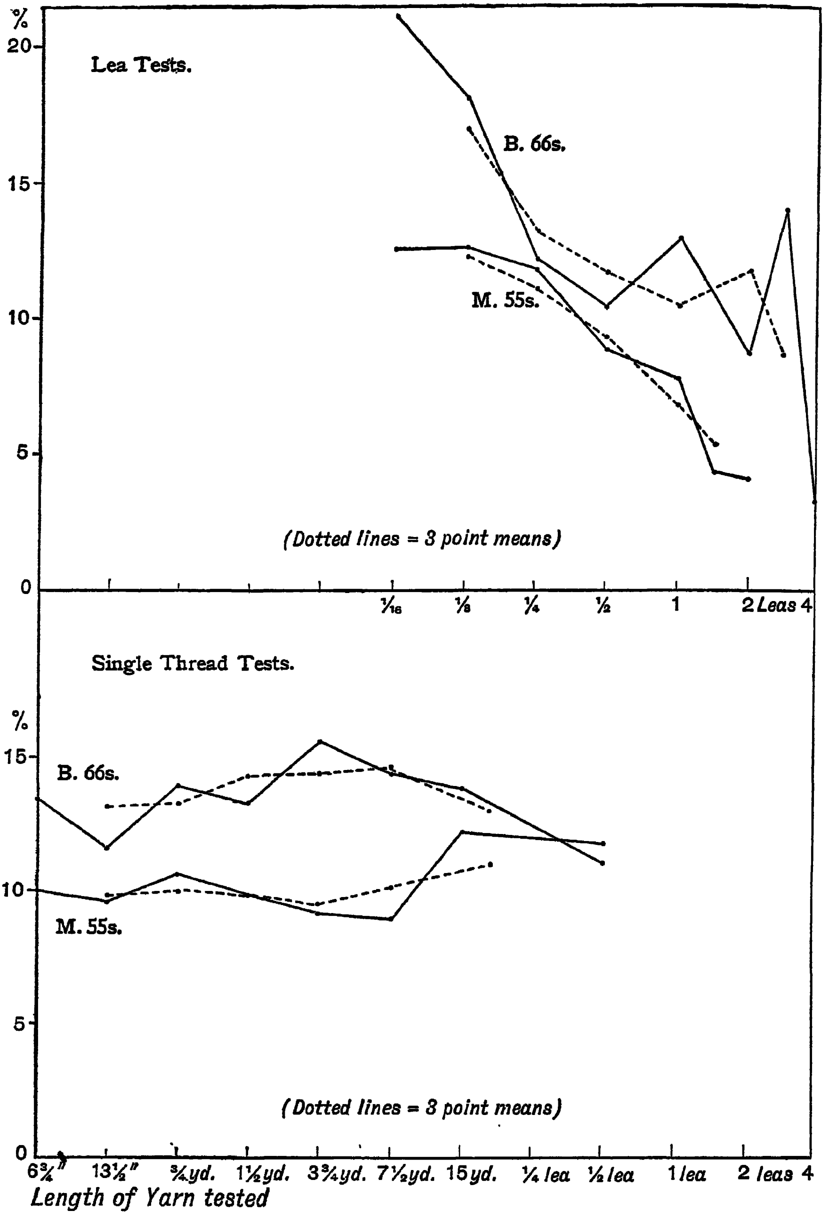


FIG. 52.—Variability of test results in Single-thread and Lea tests compared. The coefficients of variation are plotted for each of the points which represent the average values in Fig. 51. The spurious improvement in uniformity which the Packing effect introduces into the Lea test is clearly shown, as also the greater variability of weft yarn.

My general conclusion is that the lea test is a rational one, in spite of its composite nature, and that in so far as it deviates from indicating the strength of the weakest link, so those indications approximate more closely to the value of the yarn in fabric form. Having recognised its convenience, sensibleness, and reliability also, it will be well to remind the reader of the fact already noticed; that from the very nature of yarn strength it is not only possible but usual for samples of cotton which show the same lea strength to have dissimilar strength gradients (p. 185), and so to show dissimilarities when built up into fabrics.

The consistency of the lea test can be instanced from the way in which it discriminates year by year between various crop samples of the same cotton, to be quoted in the next Part (p. 242). Its blind spot is well illustrated by a comparison of two particular samples, whereof " A " had the higher Intrinsic Strength and yet gave rather lower strength in the lea test. It was found after tracing the strength gradient of these two samples that " A " had also a proportionately higher Peak Strength, but had not drafted so well; its gradient therefore fell more rapidly than that of " B," crossing the latter between the 1 and 2 metre lengths. The marked inferiority of " A " which thus resulted, by the time that the gradient had reached down to the lea length, was then much improved again, in the lea test itself, by a much bigger Packing effect; sample " A " was thus restored to a position of merely moderate inferiority. Two yarns which differ thus would seem to be suitable for very different kinds of work; to have judged " A " by its lea strength alone would have been unfair to it. This example will serve also as a final illustration of the utilitarian value which lies behind such conceptions as those of Gradient, Peak, and Intrinsic Strengths (p. 210).

The lea test is good enough to be worth the trouble of making it better. If this account should happen to stimulate any student of textile history into the making of researches on its genesis, it will be found, I think, to have been the outcome of some long-forgotten research by some founder of textile technology, who so adjusted the length tested to the circumstances of the test as to balance its liabilities by its assets.

CHAPTER XIII

THE THIRTEEN SAMPLES

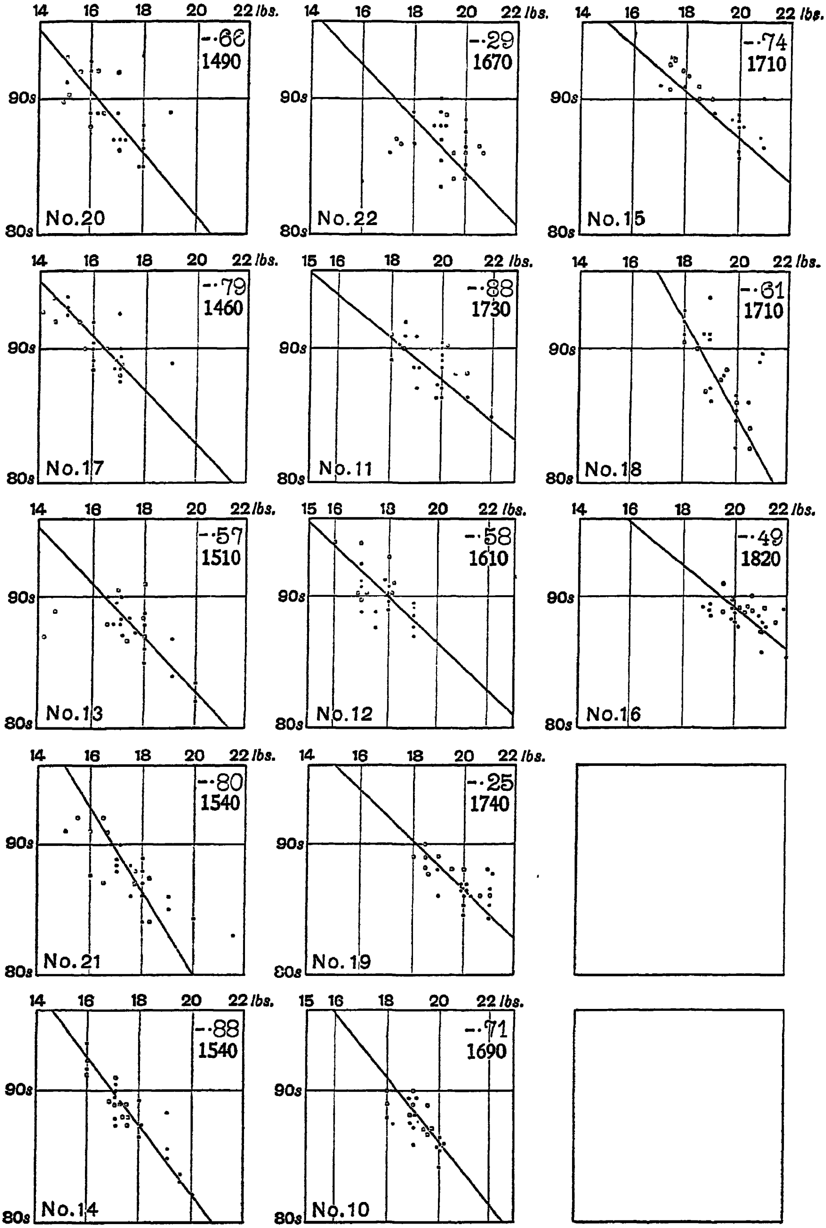
A GENERAL outline of the relationships between raw cotton and yarn has now been drawn. It remains to demonstrate that it is adequate to account quantitatively for the phenomena observed, as exemplified in my test case of the Thirteen Samples.

I have already drawn upon the data from these samples for citation in the process of drawing this outline; it might therefore appear that to return to them for this demonstration would be to argue in a circle. Consequently it is advisable to repeat my previous statement (p. 76); that although it has been convenient, for several reasons, to quote actual examples from this particular series of samples, yet the general discussion is not based upon them alone, but on the observation of yarns and cottons of every type, from 10s Indian to 300s super Sea Island.

The Thirteen Samples consisted originally of seven new strains and three commercial varieties of American cotton, with three duplicates. The three varieties were Express, Sunflower, and Polk; these names are cited merely to show that a wide range of American types was covered. Reference has already been made (p. 75) to the accidental mixture which happened during the original spinning tests, so that whereas **Nos. 16** and **17** were duplicates when they arrived, they emerged from the mill with one at the top of the list and the other almost at the bottom. From this it follows that, as quoted in this book, these are random samples of the American crop.

Regarding them simply as such, there is no doubt about their spinning behaviour. Seven separate sets of spinning tests were made on them, until all discrepancies were cleared up, and the last two spinnings in the 90s full twist here studied were in perfect agreement. It was at this stage that they came into my hands, as cops of yarn and bobbins of jack-frame rovings.

The significance of the results obtained in the present chapter



Light figures (-·88) = *Correlation Values*
Heavy figures (**1540**) = *Count Strength Product*

Fig. 53.

is mainly due to the fact that the number of samples is large enough to make any fortuitous fit of observation and interpretation very unlikely. It was the case for years that one, or two, or three of the thirteen always left their feet protruding from the Procrustean bed of any attempted explanation. So far as the Strength of Yarn is concerned, that explanation now seems to be complete; this chapter deals only with 90s twist, single and two-fold, but it contains evidence from which the properties of any other type of yarn could have been predicted, if material had been available on which to verify such predictions. No attempt is here made to analyse such yarn properties as are recorded under the headings of elasticity and twist yield; they have been sampled by measurements of the hair properties in stress-strain, but the data are not sufficiently exhaustive for quantitative employment.

In the matter of interpretation it is important to note that mass computation alone is not necessarily competent to provide explanations. There may be "limiting factors." That is to say, that some factor may impose a limit which cannot be passed, though variations controlled by it can take place until that limit is reached. Thus, the intrinsic strength forms a limit to average local strength of yarn, while the degree of approach to this limit may be limited by other factors instead.

The presentation of the data requires more explanation. It would be quite inappropriate to such a short series of thirteen to employ rigid treatment by correlation methods only. If simple inspection methods are used instead for all the preliminary work, then it is difficult to compare many sets of figures, even in graphic form, unless precautions are taken to ensure that the comparison is quantitative; for example, a perfectly smooth progression of the thirteen from 98·8 to 100·0 would be insignificant in comparison with a staggering progression from 50 to 150. The "percentage change" is the important thing, and this can be seen quite easily by making use of semi-logarithmic paper; the swing and slope of any graph plotted on such paper is dependent on the relative changes, not on the absolute values. Thus, in one and the same plotting arrangement on such paper, the change from one sample to another would always be represented by a line sloping at the same angle, whether the change were one of staple length from $1\frac{1}{8}''$ to $1''$, or of hair weight from 0·00130 mg.

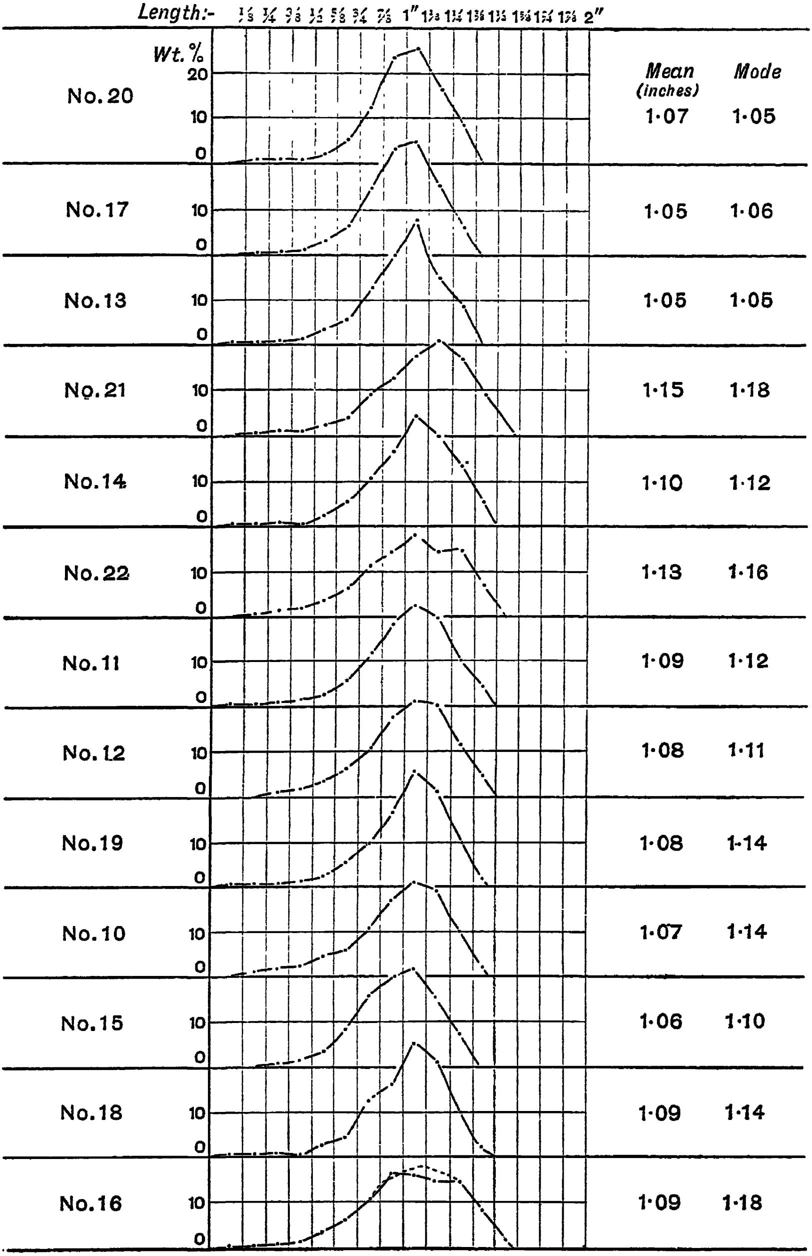

Length:- 1/8 1/4 3/8 1/2 5/8 3/4 7/8 1" 1 1/8 1 1/4 1 3/8 1 1/2 1 5/8 1 3/4 1 7/8 2"
Wt. %
20
10
0
Mean (inches)
Mode
No. 20 1·07 1·05
No. 17 1·05 1·06
No. 13 1·05 1·05
No. 21 1·15 1·18
No. 14 1·10 1·12
No. 22 1·13 1·16
No. 11 1·09 1·12
No. 12 1·08 1·11
No. 19 1·08 1·14
No. 10 1·07 1·14
No. 15 1·06 1·10
No. 18 1·09 1·14
No. 16 1·09 1·18

FIG. 54.

to 0·00116 mg., or again, if it were a change from a lea test result of 17 lbs. to another of 15 lbs. 5 oz. In all three cases the proportionate change is the same, and consequently the slope of the line is unaltered. Hence it follows that the slope will be unaltered by multiplication or division.

One caution against over-confidence in the simplicity of this method may be needed. Two such semi-log (or arithlog) curves may have very different slopes, and yet be fundamentally identical, if the dimensions which they record are not identical. Thus, measurements of yarn diameter might change from 2 to 3; parallel measurements of yarn weight would show a change from 2^2 to 3^2, or from 4 to 9. The change from 2 to 3 would give a very different slope from that given by a change from 2 to 4½, though the latter would have the same slope as 4 to 9. The result of squaring a set of data is to double the amplitude of their plotting by this method, of cubing to treble it, and of raising to the fourth power is to quadruple it. The consideration of dimensions is therefore important.

In order that this simple device of comparison in semi-logarithm plottings may be used conveniently, the Thirteen Samples have invariably been considered in a definite order. Any order would serve, but I first arranged them in the order given by one of the lea-test determinations of yarn strength, and this order is maintained throughout, namely:

(Nos.) 20. 17. 13. 21. 14. 22. 11. 12. 19. 10. 15. 18. 16.

This particular order has the advantage of indicating the merit of the series, as judged by the spinner from the lea tests, by the upward slope of a line from left to right.

Not all the data which have been obtained while studying these samples are presented here, but none have been omitted which have any pretensions to accuracy. The degree of variability of the measurements is frequently stated or shown, and in most other cases it has been determined. Where no explicit information is given it may be taken for granted that the difficulties of measurement have been fairly well overcome, so that the value stated is within 5% of the true value.

My discussion of the data will be conducted by direct reference to the diagrams. This is in deliberate contrast to the preceding chapters, where it has been my aim to make the discussion

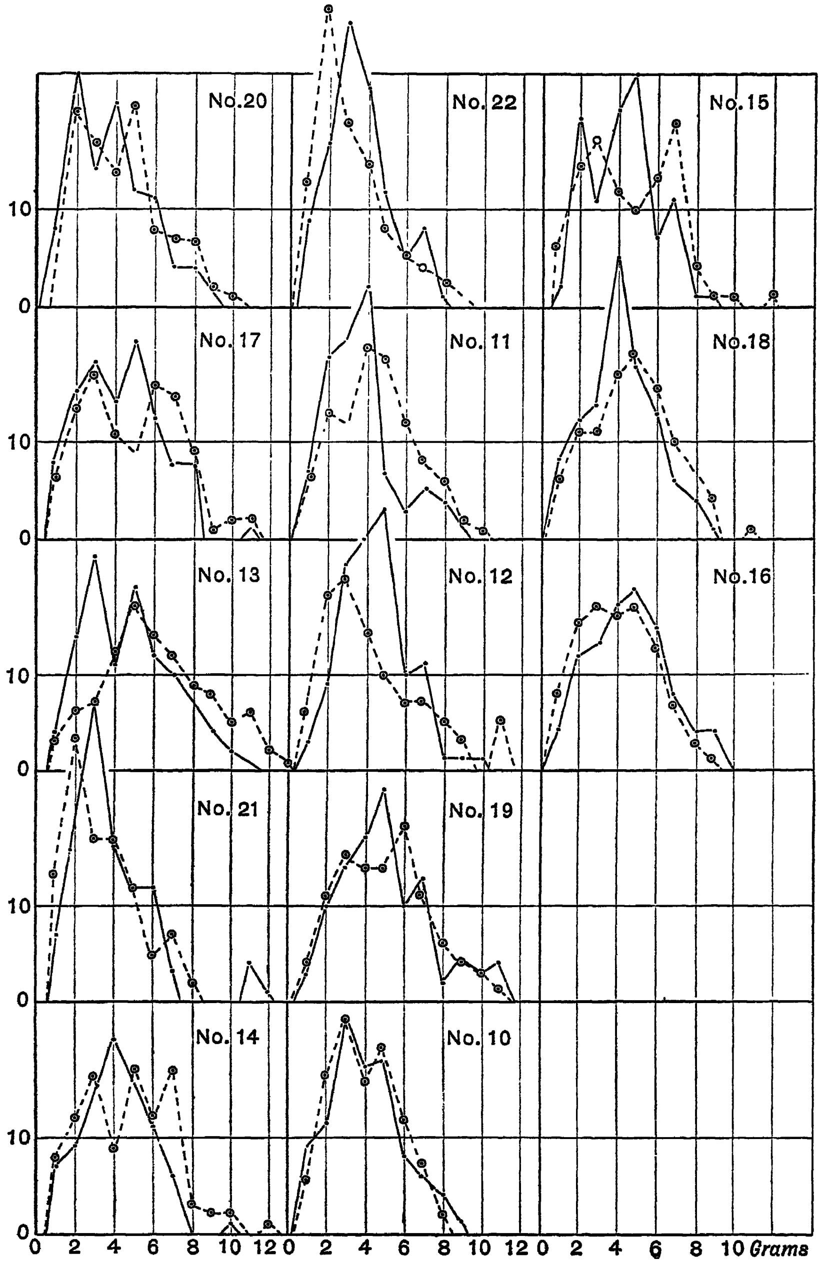

No. 20
No. 22
No. 15
No. 17
No. 11
No. 18
No. 13
No. 12
No. 16
No. 21
No. 19
No. 14
No. 10
10
0
0 2 4 6 8 10 12 0 2 4 6 8 10 12 0 2 4 6 8 10 Grams

FIG. 55.

general, using the text to express my own interpretations, and leaving the diagrams to show the actual facts without too much prejudice. The reader should turn to Fig. *60*, the first of the semi-log plottings, in order to begin the study of the data; Figs. *53* to *59* and Fig. *103* are special plottings, and will be brought into the discussion as they are needed.

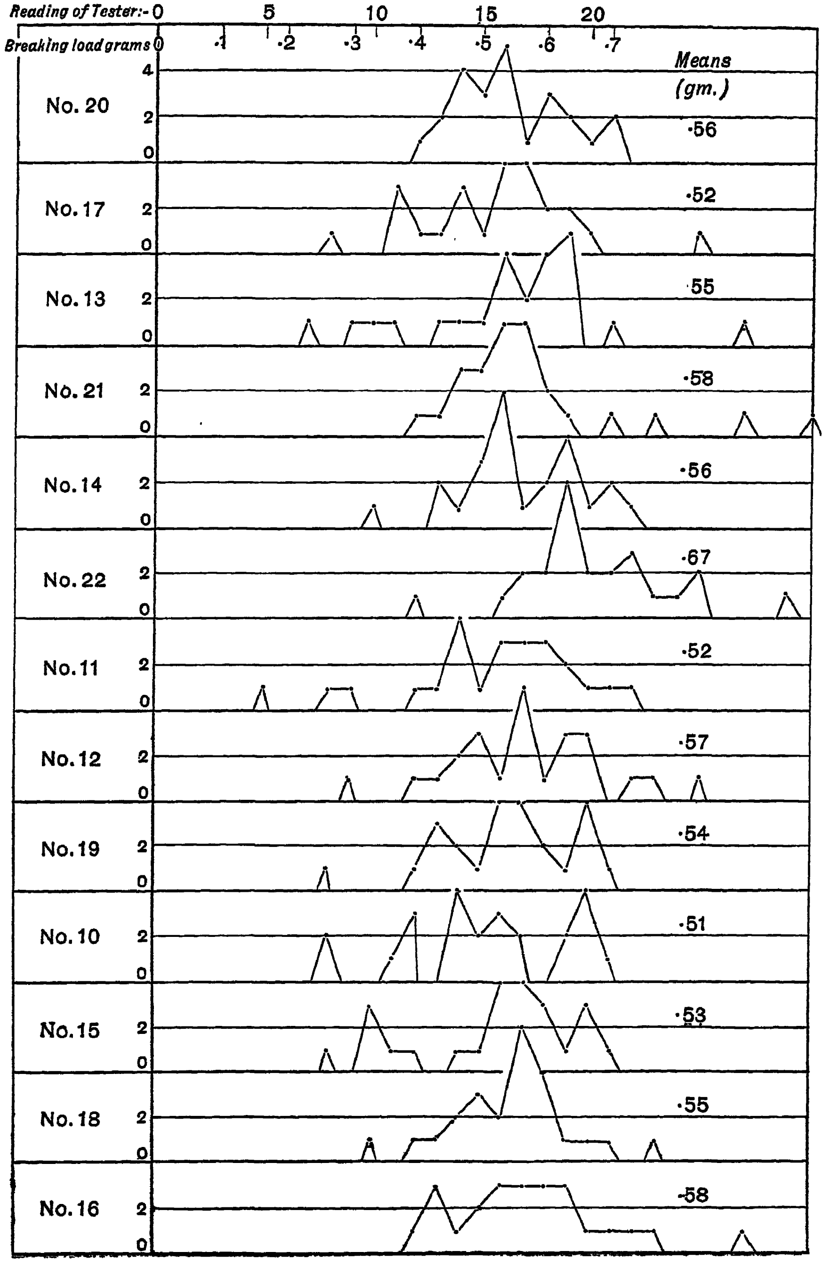
The Cotton

The order in which the hair properties will be taken is determined merely by convenience in grouping the diagrams.

Hair convolutions (p. 69).—The number of convolutions per centimetre in Fig. *60* will serve as an illustration of the method of comparison. Above this figure is the sloping line which represents the sequence of lea strengths, redrawn from the later Fig. *79*. The convolutions at first have a trend in the same direction, but the right-hand half of their curve ceases to rise. If these two curves had to be considered by themselves, without all the other supplementary evidence from other characters, this might be read to mean that the number of convolutions limited the lea strength until 65 per cm., after which some other factor became limiting. Actually there is no indication anywhere amongst these data that this variation in the number of convolutions has any effect on the yarn properties at all. Such importance as the convolutions may have in these samples must be quite secondary.

Hair Break (p. 67).—The hair-breaking strength shown in Fig. *61* appears to be even less relevant. Details of the frequency-distributions for two separate groups of a hundred breaks each are given in Fig. *56*. The averages plotted in Fig. *61* are those of three hundred hairs from each sample. The very wide range of the latter curve should be noted, from 3½ to 5 grams. Further consideration of this curve can be postponed until the hair weight is available, when it will be used to compute the " hair break per equal weight of hair," which is termed the Intrinsic Strength. It will then be found very relevant indeed.

Hair Cross-section (p. 64).—The wall-thickness data in Fig. *62* are only approximate (p. 66). Those for ribbon width in Fig. *64* are more exact, but both are undistinguished. They show a general tendency for fineness by size to follow the fineness by weight which is shown between them in Fig. *63*.



Reading of Tester:- 0 5 10 15 20
Breaking load grams 0 ·1 ·2 ·3 ·4 ·5 ·6 ·7
Means (gm.)
No. 20 ·56
No. 17 ·52
No. 13 ·55
No. 21 ·58
No. 14 ·56
No. 22 ·67
No. 11 ·52
No. 12 ·57
No. 19 ·54
No. 10 ·51
No. 15 ·53
No. 18 ·55
No. 16 ·58

FIG. 56.

Hair Weight (p. 66).—This, on the other hand, is the most accurate measurement of all, and the points plotted in Fig. *63* are not more than 1% removed from the true values under similar conditions of atmospheric humidity. There is a general trend which slopes down to the right as a reciprocal of the lea test, so that the finer the cotton the stronger the lea. But there are exceptions to this, notably sample **No. 22**, which is the finest but one among the whole thirteen, and yet makes weaker yarn than most of them. Any interpretation must include this marked exception to what is otherwise a suggestively general tendency.

A reciprocal of the curve of Fig. *63* is plotted just over it as a dotted line. This shows the average number of hairs in cross-section of 90s yarn, this being the count to which these samples were spun; it is obtained by dividing the hair weight per centimetre into the yarn weight per centimetre. The range is from 32·3 to 43·2 hairs.

Hair Slipperiness (p. 69).—This has to be measured indirectly. The pull required to break the untwisted roving is shown in Fig. *65*. Having regard to the importance of this test I have shown the detailed frequency-distributions of each individual test in Fig. *56*, just as they were shown for the hair break also.

Below the averages of these frequencies in Fig. *65* is plotted again the number of hairs in cross-section of 90s yarn, as Fig. *66*; this has already been shown as a dotted line in Fig. *63*. If the number of hairs in 90s yarn is multiplied by $\frac{90}{15}$ it becomes the number of hairs in cross-section of the 15-hank roving from which these tests were made; the form of the curve is not affected thereby.

Now it is clear that there is a strong resemblance between Figs. *65* and *66*; between the pull required to break the roving, and the number of hairs therein; the finer the cotton the stronger the roving. But the parallel is not exact; thus it will be noticed that when the two finest samples are compared with one another, the one which has the weaker roving makes the stronger yarn.

This discrepancy is investigated further in Fig. *74*, where the untwisted roving strength is divided by the number of hairs concerned in each sample, thus giving a value which expresses the "slipperiness" of the individual hairs. The dividend curve has a peculiar form; the reader should notice it carefully, as it will only be met with twice more. It is fairly steady among the

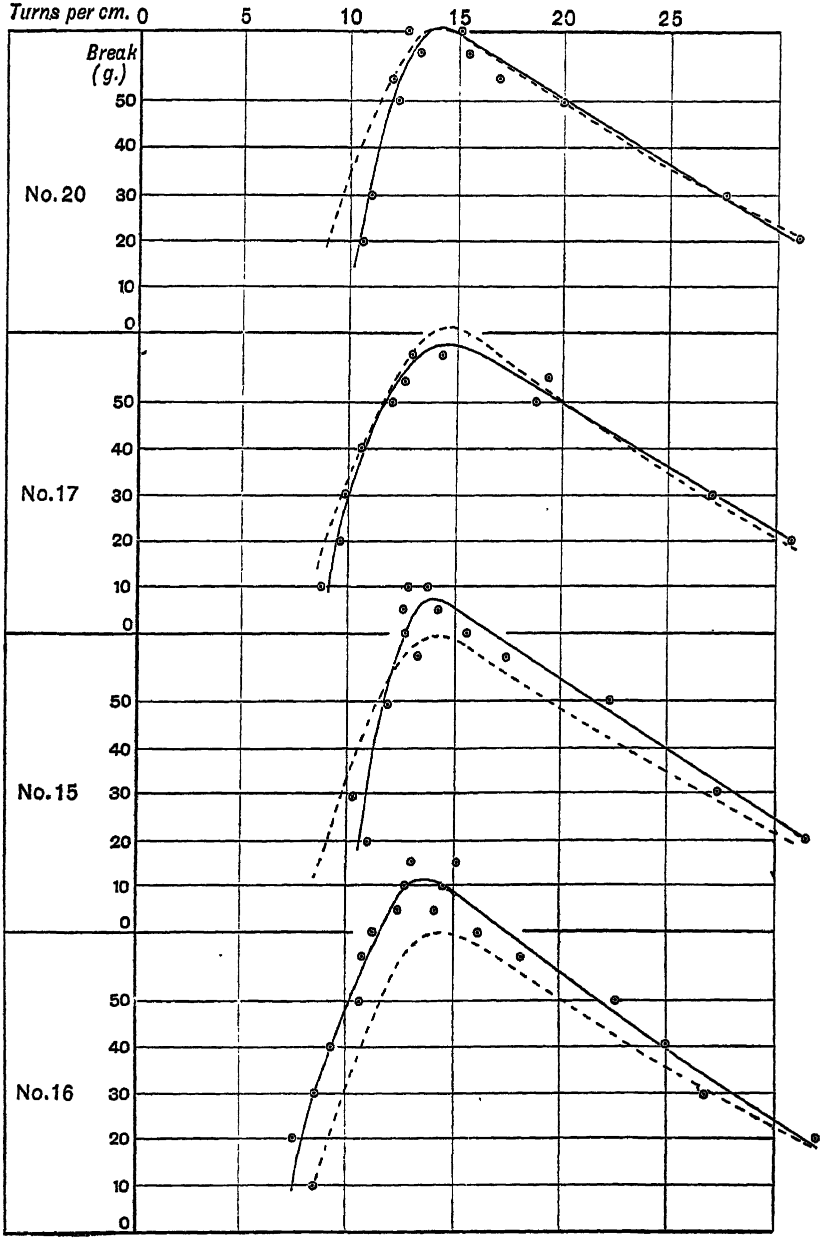

Turns per cm.
0
5
10
15
20
25
Break (g.)
No. 20
No. 17
No. 15
No. 16
50
40
30
20
10
0

FIG. 57.

weak-lea samples to the left-hand side, and then falls away to the right. The strongest leas have the most slippery hairs.

For the moment it can only be regarded as a hint, just as in the case of the convolutions, but the hint is a broader one in this case because the aberrant fine sample **No. 22** is now included in the steady left-hand portion. Also, the hint fits in with my second paradox (p. xvii), that cotton needs to be slippery until drafting is finished, but sticky afterwards.

Although the data for cross-section of the hair are hardly good enough to stand calculation of the kind, yet for the sake of

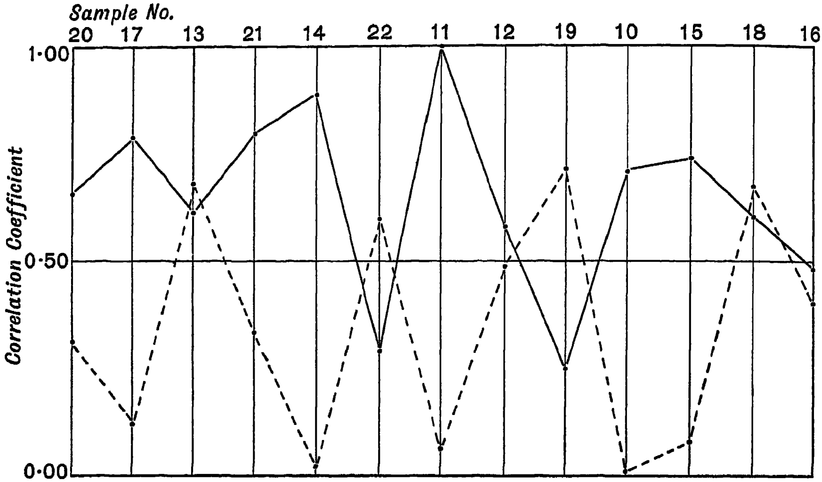
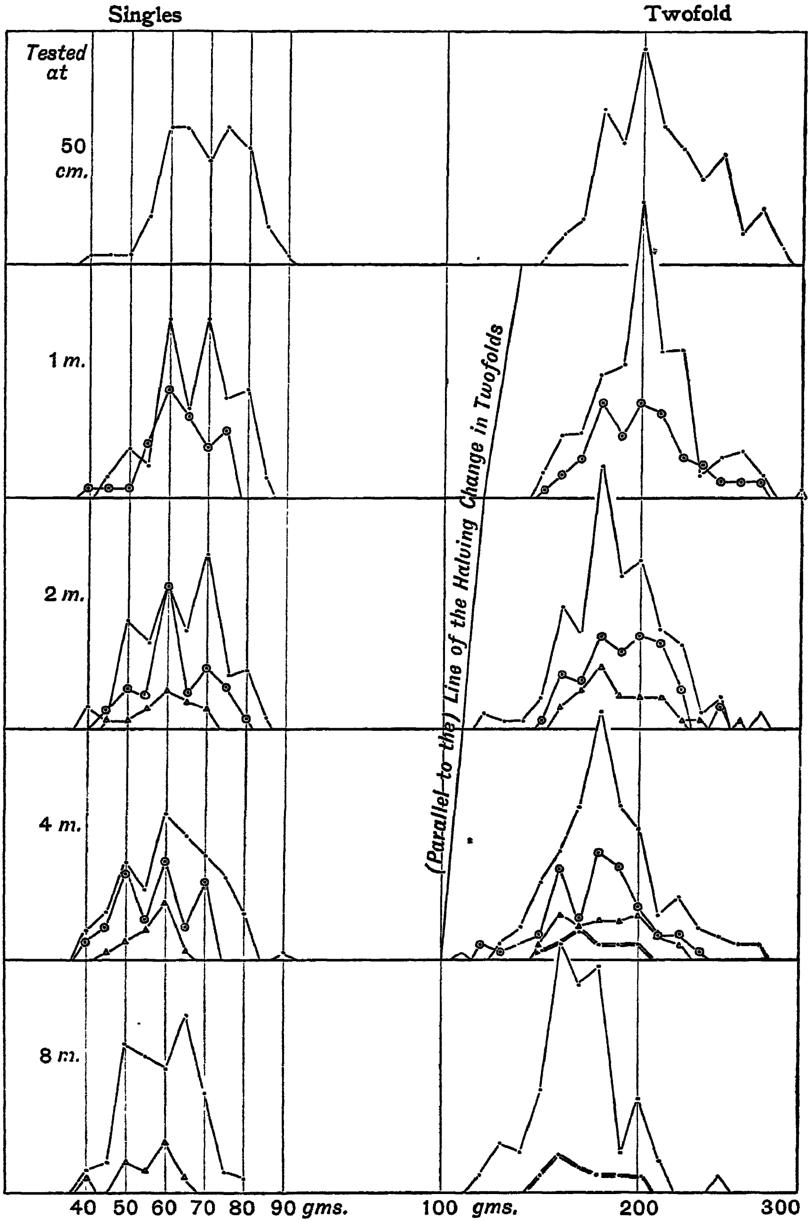


FIG. 58.

completeness I have plotted Fig. *67*, which shows the rough computation of the hair surface per gram. This disregards the possible effect of the convolutions upon the " effective surface," as distinct from the total surface here shown. The chief interest is in the absolute values shown, which are in the neighbourhood of half a square metre per gram of cotton.

I have treated hair slipperiness at some length, on account of the imperfections of the method by which it has to be measured. It should be emphasised that its presentation in Fig. *74* indicates the actual " working slipperiness " of the hairs as used in the mill during drafting, and as influencing adhesion when twisted up into yarn. The results of attempts to analyse this slipperiness



⊙ *Predicted to double length.* ▲ *Predicted to four fold length.* ━ *Predicted to eight fold length.*

FIG. 59.

are not worth including here; the factors concerned are the surface area, inter-surface friction, nature of the contact, whether on the crests of convolutions only, compressibility of the hair and of the mass of hairs, and the pressure applied.

Intrinsic Strength (p. 67).—This character is of such importance as to need a separate section, although it is merely a computed curve, derived from the data of hair break and hair weight already presented in Figs. *61* and *66*. As represented in Fig. *69* it has noteworthy features. There is a general similarity of trend to the lea-strength curve of Fig. *79*, but there are again several striking departures from similarity when the behaviour of individual samples is considered. The most significant fact, however, lies in the steep inclination of Fig. *69* as compared with Fig. *79*; the range of intrinsic strength is far greater than is needed to explain the small range of lea strength. This is largely on account of the fact already discussed (p. 209), that the available difference between the best and worst samples is smoothed out and partly wasted in the lea test, by the action of readjustment chiefly.

Hair Length (p. 60).—The comparative insignificance of hair length is shown most emphatically when Fig. *71* and its neighbours are compared with the intrinsic strength changes in Fig. *69*. The negative result is of such importance (Chaps. I and IX) in its negativeness that I have presented the complete frequency-distributions for each sample in Fig. *54*; these are the sorter curves. From them are derived the modal lengths shown in Fig. *71*, which an earlier chapter indicated as being the most important length (p. 156). The approximate maximum length is plotted separately, however, in Fig. *70*, and the mean lengths are shown on the former as detached crosses. The "regularity" of length is shown in Fig. *68*, by treating the sorter curves as if they were normal curves of error, and computing the percentage variability.

Whether the conclusion be reached by a casual glance at these data, or by ten years' examination of them, that conclusion can only be this: that length of staple is practically irrelevant to the spinning behaviour of these samples, as expressed in the lea tests on 90s twist.

Moreover, the general trend of Fig. *68* shows the more variable samples to be rather better than the less variable ones. Both conclusions are almost as heretical as they could be.

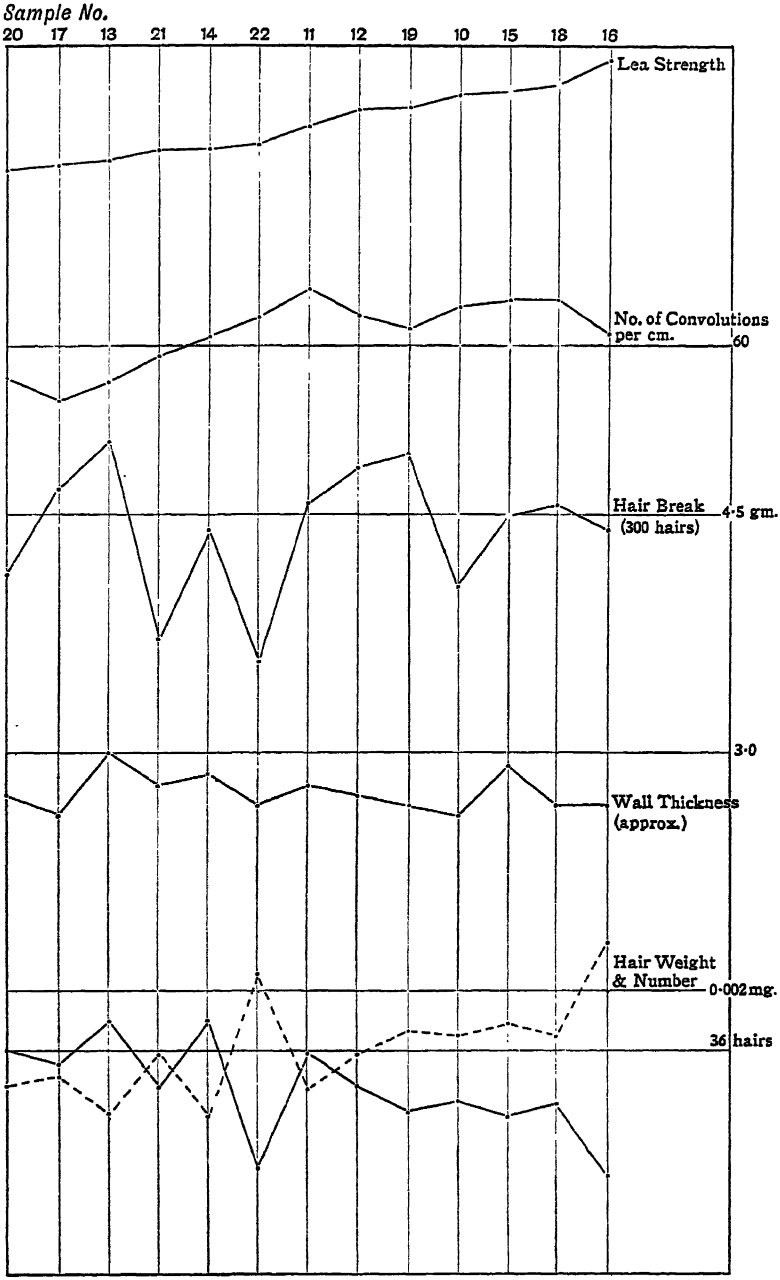


FIG. 79

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63

The importance of length might be partly rehabilitated if it acted through a third or fourth power of its own linear dimension, but even this cannot be allowed to it, for Chap. IX has shown that its effect on strength is merely direct.

Although three of the shortest samples give the three weakest leas, yet one of the three samples which have the strongest leas is identical in length with that which gives the weakest lea of all; these are **No. 15** and **No. 20** respectively. The case of **No. 15** shows that the short cotton can become one of the best, even when spun into unduly fine counts, if its shortness is compensated by other characters; these range, moreover, far more widely than length. Similarly, although **No. 22** has both length and fineness as well, yet these assets are unable to balance its low intrinsic strength and its stickiness. It will be seen later that strong two-folds can be obtained from these short samples.

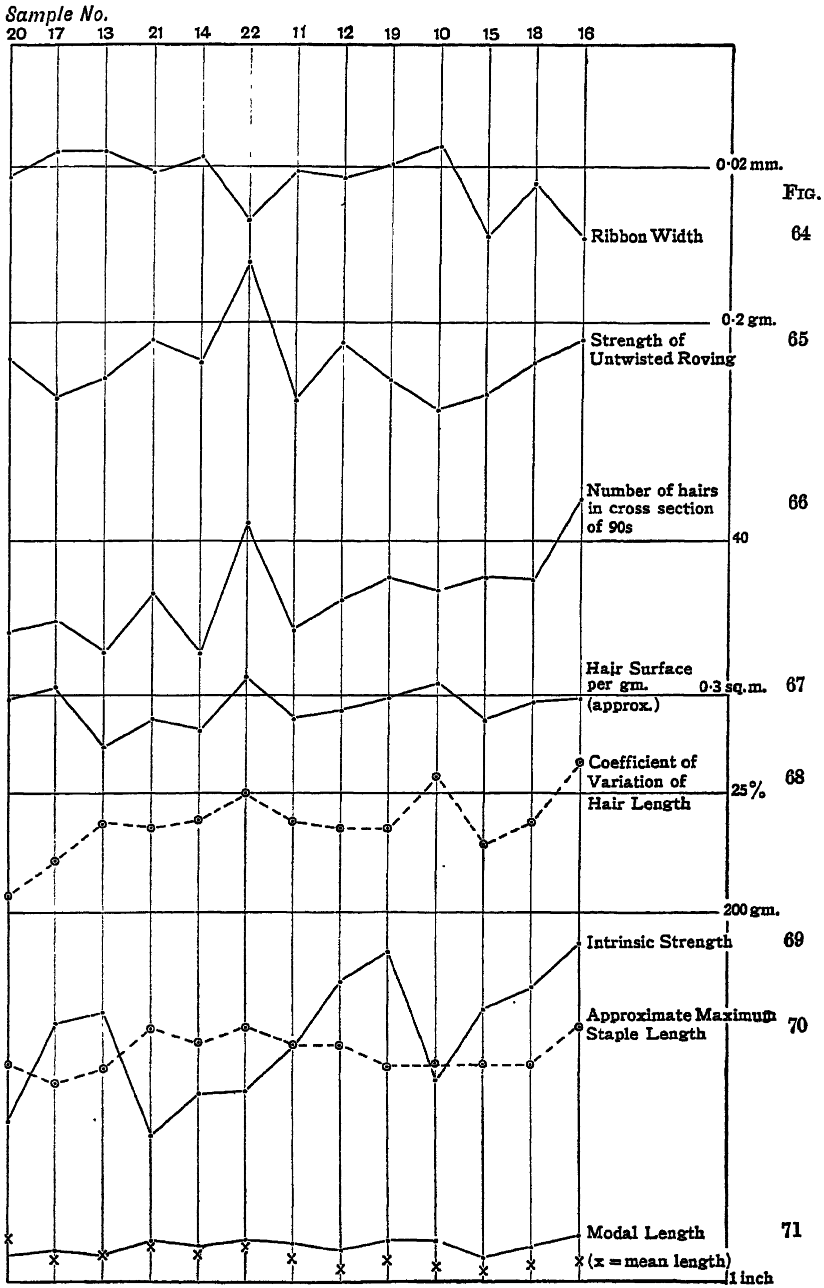
The reader's attention may here be directed to a curious result appearing later on, where the Coefficient of Restitution in impact tests on the yarn is found to resemble the hair length. It is the only character which does so.

Hair Content of Wax and Ash.—The wax extractions were made with ether on strictly limited amounts of material; they look in Fig. *73* as if more accurate determinations than were possible might have shown something of interest. The ash-content curve in Fig. *72* has a curiously suggestive resemblance to the hair-break curve of Fig. *61*.

The Yarn, in General

Elsewhere in this book I have stated that the main difficulty confronting my early attempts to relate the properties of cotton with those of yarn was presently found to consist in our inadequate knowledge of the yarn itself (p. 79). The reader has now seen that bold variations exist amongst the hair properties of the Thirteen Samples. It may help him to re-create something of the atmosphere of bewilderment with which my early researches were surrounded if I begin by presenting the kind of data upon which I was at first dependent for the gentler variations of yarn strength.

The Arbitrary Assessment of "Strength."—That the strength exhibited by these yarns was not far from the optimum, so far


Sample No.
20 17 13 21 14 22 11 12 19 10 15 18 16
0·02 mm.
FIG.
Ribbon Width 64
0·2 gm.
Strength of Untwisted Roving 65
Number of hairs in cross section of 90s 66
40
Hair Surface per gm. (approx.) 0·3 sq.m. 67
Coefficient of Variation of Hair Length 68
25%
200 gm.
Intrinsic Strength 69
Approximate Maximum Staple Length 70
Modal Length 71
(x = mean length)
1 inch

as the twist was concerned in controlling it, was ascertained by Twist-yield tests (p. 175), four of which are depicted in Fig. *57*; they will be discussed briefly afterwards.

The natural starting-point for assessment of the relative strengths was the lea test (p. 233), whereof the results for the two final and separate spinnings in 90s twist are plotted together in the correlation diagrams of Fig. *52*. The two spinnings agreed excellently. The correction of the observations to a common count had then to be made (p. 222).

Fortunately, the variation in count was not great, as Fig. *75* shows, but it was enough to interfere with the exactitude required, for the average difference in strength from sample to sample was little more than a quarter of a pound. A line which indicated the most likely position of the correlation axis was fitted on the correlation diagram, and the intersection of this line with the nominal count was taken provisionally to be the " strength " at 90s. The order of arrangement already defined (p. 244) was thus decided, and the " strength " in this order is that shown by Fig. *79*.

Though the position of the correlation axis can be ascertained with some certainty on such diagrams as those of Nos. 14, 15, and 18, it is not more than guess-work on some of the others. Consequently it is little more objectionable to assume a constant correlation, and use the Count × Strength product (p. 225). The result of doing this is shown in Fig. *80*; the general trend is the same as before, but there are differences in detail which interfere seriously with the attempt to compare " strength " with the hair properties.

Such a curve as that of Fig. *82* may be taken as typical of the results obtained when the single-thread test was substituted for the lea test, using some arbitrary length, such as the 360 cm. of this particular example. Fuller examples are given in Fig. *105*, for this length and for 65 cm. also, both having been taken on the same automatic tester (p. 348), and presented without any correction for count.

Below this last figure is shown the result of testing in such short lengths as are now known to be vitiated by the clamp effect (p. 215); all trace of resemblance to the original order is disappearing. Lastly, the average strengths of the two-folds which were made many years later (p. 186) can be seen in Fig. *112* to

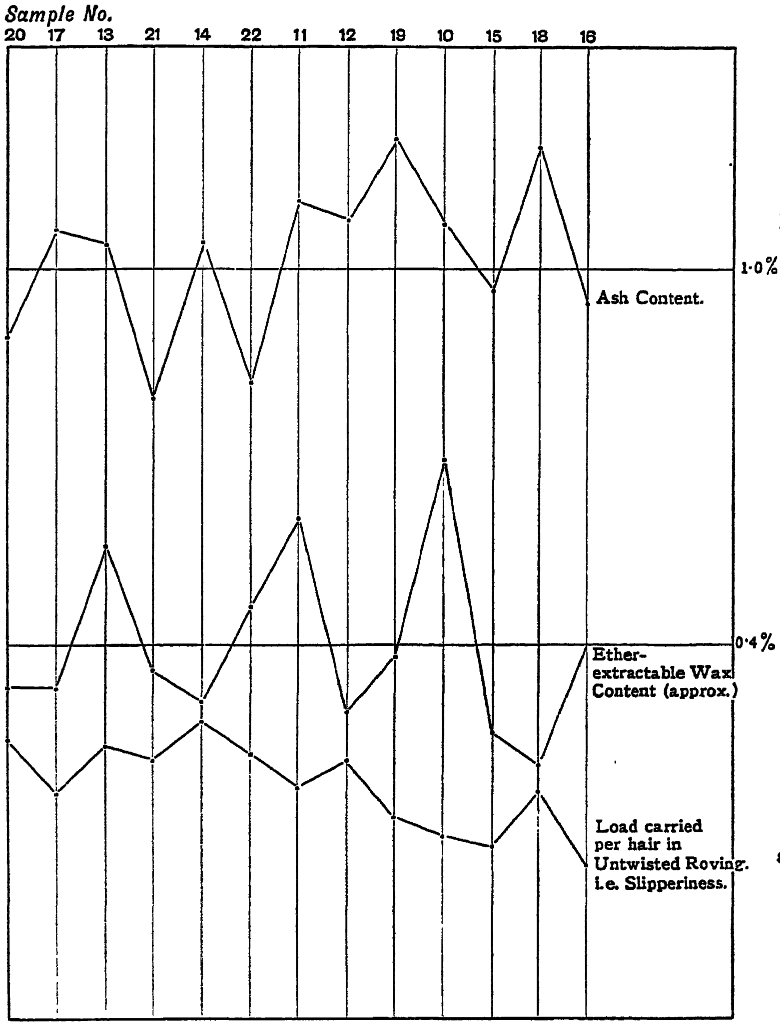


FIG. 72

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74 and 96

have a partial resemblance to the intrinsic strength curve of Fig. *69*, and to be even less like the original lea-test line.

Bringing all these data together in this way, the reader can see that some such clarifying conceptions as those of yarn gradient and the like are self-evidently necessary. While these data were being collected, their effect was one of sheer confusion; "strength" might be anything. To attempt to match the hard and definite facts obtained for the hairs against such amorphous and elusive values as the yarn provided seemed waste of time, and the whole research had to be diverted to the unexpected task of studying yarn as yarn.

Meanwhile, it may assist the reader to see from Fig. *81* that the average single-thread strength over a range of various lengths, which I have labelled as "likely values," is broadly similar to the lea-test results.

Lea Strength in Weft (p. 184).—Other spinnings (p. 75) had been made from the same roving bobbins into 96s soft weft, but the yarn had not been preserved, and only the limited number of lea tests made in the mill were available. These are shown in Fig. *78* in the form of percentages of the lea strength given by the usual twist yarns made and tested in the same mill at the same time. Only the general trend of the data can be trusted, though this shows clearly that the general trend of the weft strengths is like that of the twist strengths; the stronger the twist yarn, the higher the strength of the weft yarn.

Impact Tests (p. 229).—One of these is shown in Fig. *76* to introduce quite another kind of assessment of strength. Their significance in relation to hair slipperiness and yarn extensibility has already been discussed; the curves illustrating this, and the effect of changing the length impacted, are Figs. *93* to *96*.

Variability of Yarn "Strength."—The proper discussion of this matter will follow automatically when the Yarn Gradient is considered. Meanwhile an early experimental result will serve to illustrate the confusion, by plotting in Fig. *85* the coefficient of variation in a set of single-thread tests made at 360 cm. on the automatic tester. The stronger the lea, the more variable is the break. An attempt to analyse this set of 1,300 tests is shown in Figs. *80* to *82*, where the five strongest breaks and the five weakest are averaged separately from the mean break for each sample; they merely re-state Fig. *85*, showing that all

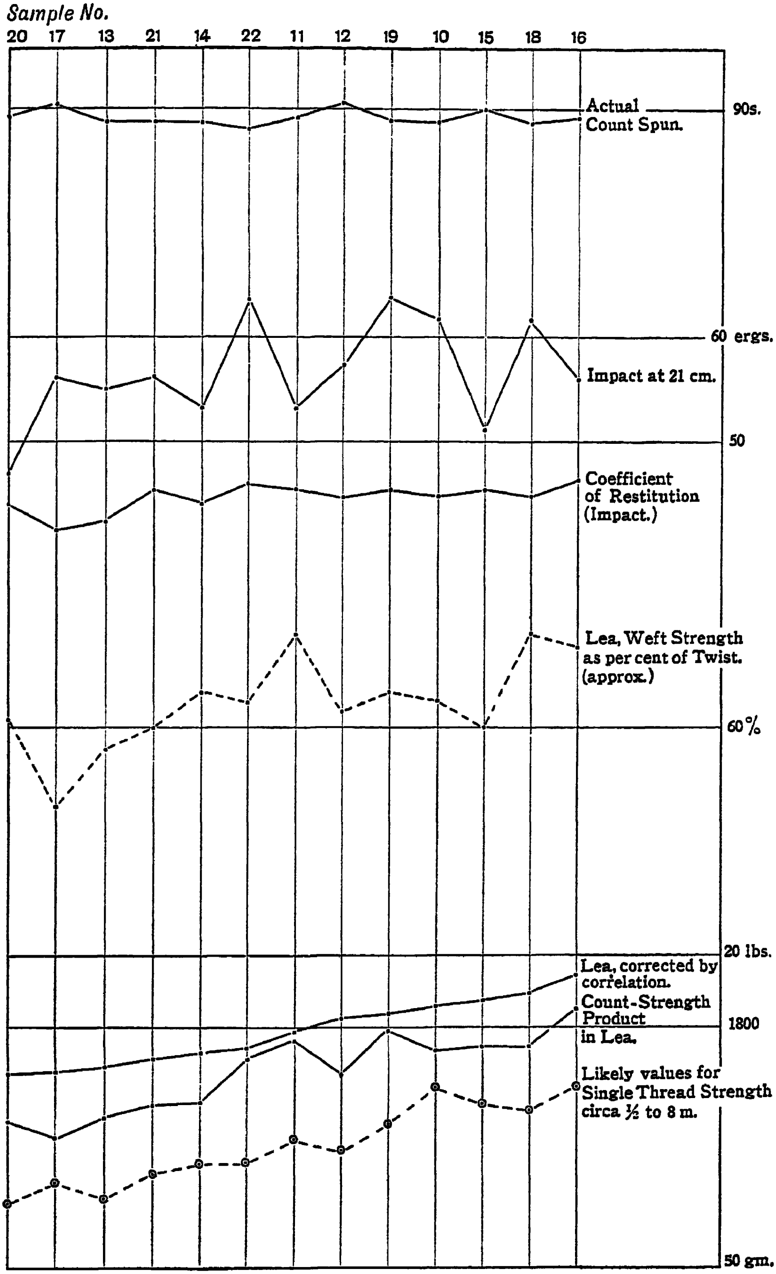

FIG.
75
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77
78
79
80
81

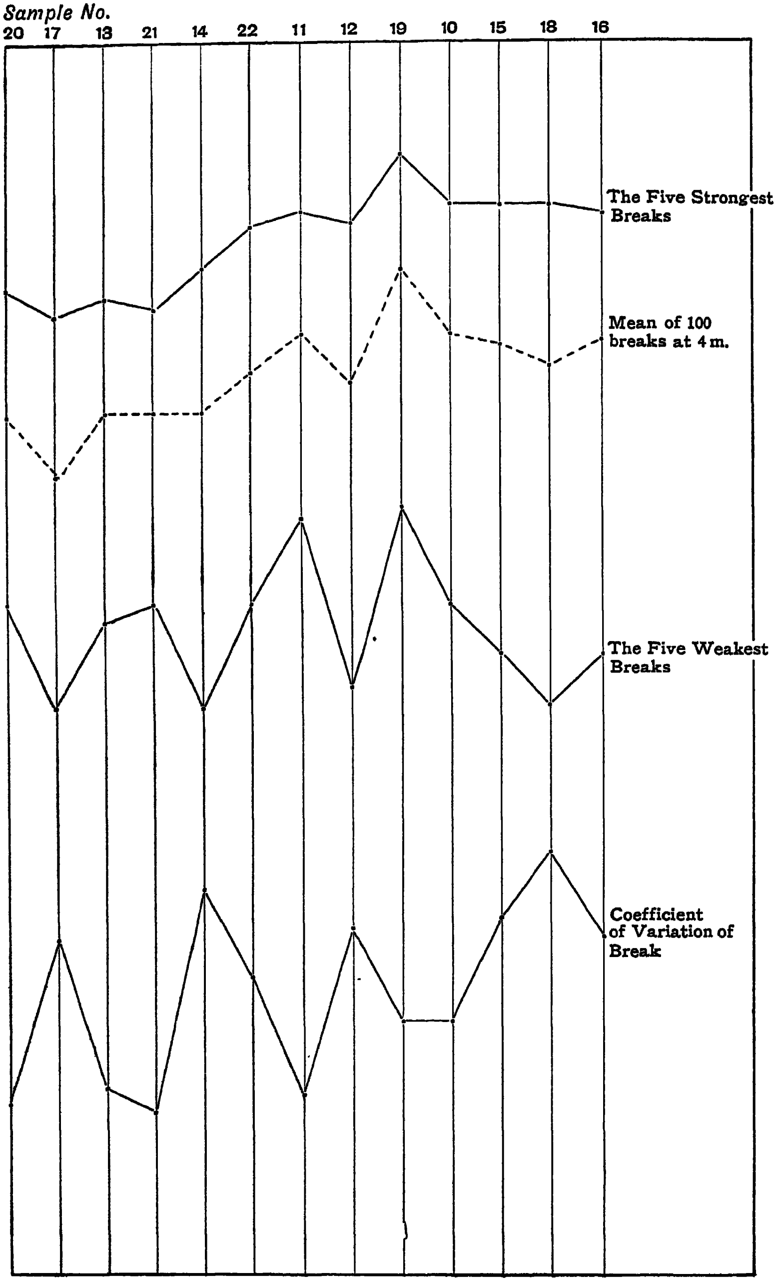
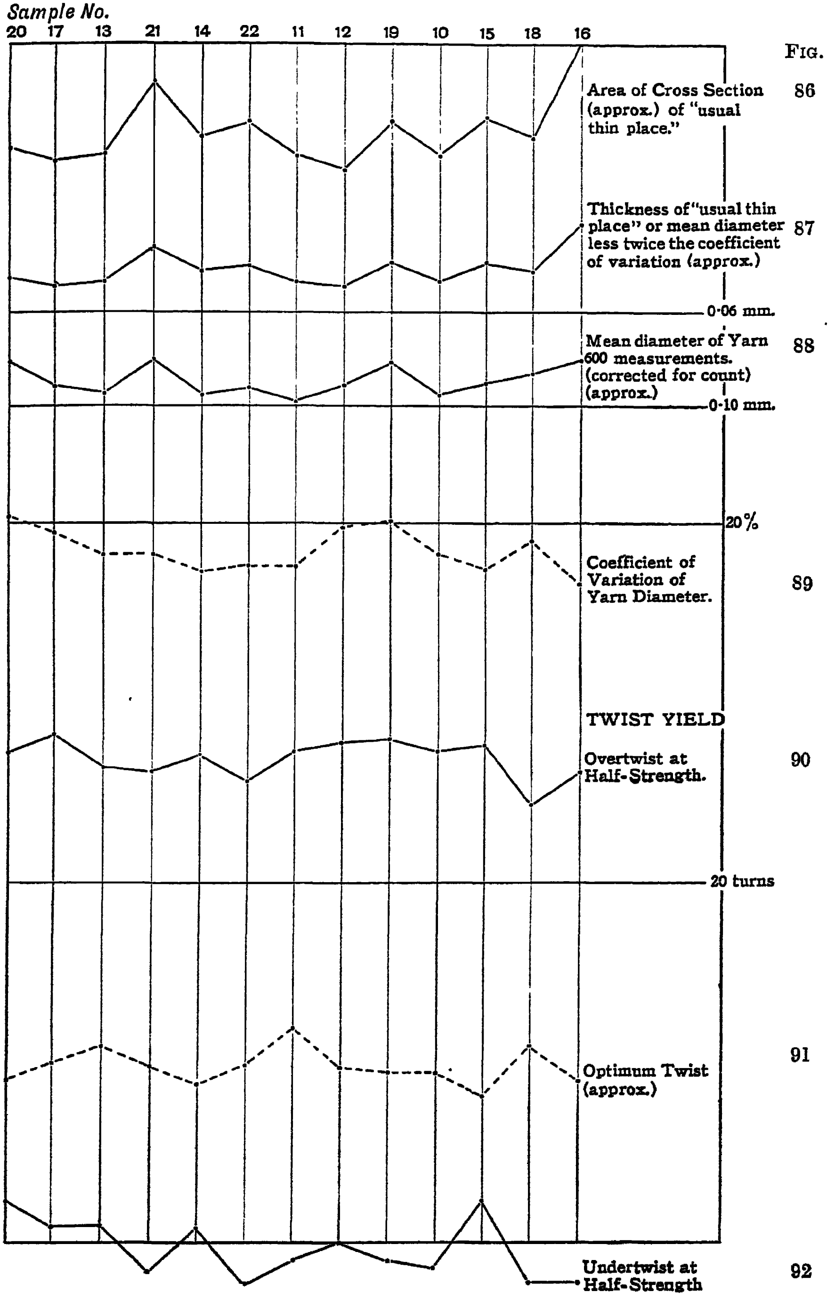


FIG. 82
83
84
85


Sample No.
20 17 13 21 14 22 11 12 19 10 15 18 16
FIG.
Area of Cross Section (approx.) of "usual thin place." 86
Thickness of "usual thin place" or mean diameter less twice the coefficient of variation (approx.) 87
0·06 mm.
Mean diameter of Yarn 600 measurements. (corrected for count) (approx.) 88
0·10 mm.
20%
Coefficient of Variation of Yarn Diameter. 89
TWIST YIELD
Overtwist at Half-Strength. 90
20 turns
Optimum Twist (approx.) 91
Undertwist at Half-Strength 92

samples are equally likely to have weak places, but that only the strong can have very strong places; thus the variability of the strong samples must be the greater.

But earlier chapters have shown that this result is artificial. It is the effect of nip-ratching (p. 94), and the true variability of the yarn is concealed below it.

Variability of Yarn Diameter.—Some early attempts to elucidate this variability by measuring yarn diameter, before the systematic research with the Harp apparatus (p. 133), are shown in Figs. *88* and *89*. Yarn diameter measurements were made on short weighed lengths of yarn, and although the data are quite inadequate they may serve to illustrate how the "thin place liability" can be assessed. The statistical definition of the coefficient of variation is such that the chance of any place being thinner than the mean by more than twice that coefficient is about thirty to one. If the coefficient be twice subtracted from the mean, the residue is therefore a rough guide to the more likely kind of thin place. This has been done in Fig. *87*, and this again is squared in Fig. *86* to give the relative cross-sectional areas of such places.

These actual data are, however, mere approximations. They are only cited for illustration of a practicable method, and the subject will recur in Fig. *119*.

Twist-Yield (p. 175).—Four of these curves are depicted separately in Fig. *57* in order to give a general impression of them; the four are selected to show the widest variations of form observed within the series of thirteen. In Fig. *91* is shown the "optimum" twist as inferred from the examination of these curves; previous chapters have dealt with the degree of uncertainty attaching to this estimation, and I therefore make no attempt to analyse it further. The values for "half-strength," both when untwisting and when over-twisting, are shown in Figs. *90* and *92*; they look as if both length and fineness were implicated, which might reasonably be expected.

Stress-strain (p. 189).—The detailed Elastometer records for each sample are not worth reproduction in full, for my present purpose, and a summary will suffice. These records were taken on one-metre lengths.

Total Extension at 10 grams load is shown in Fig. *97*, that at 50 grams has been given in Fig. *93*, while the 60 gram values are shown in Fig. *98*.

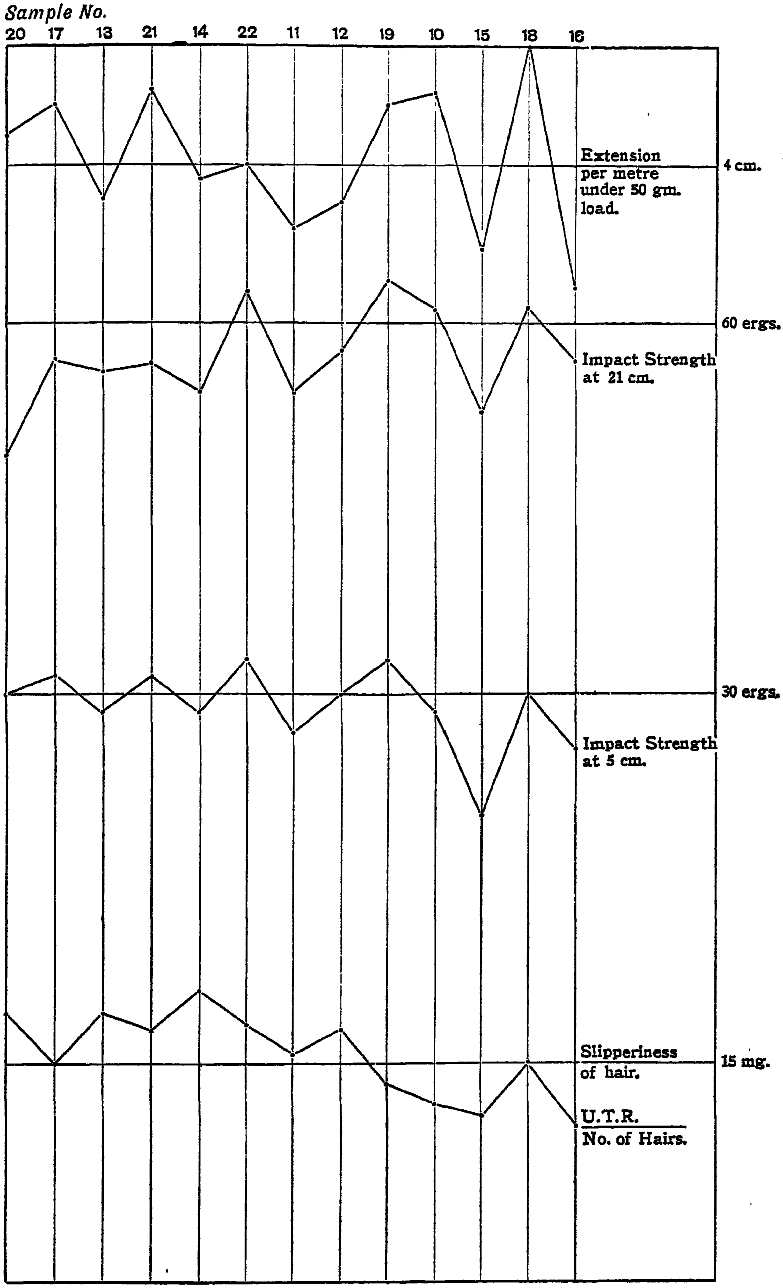


FIG. 93

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96 and 74

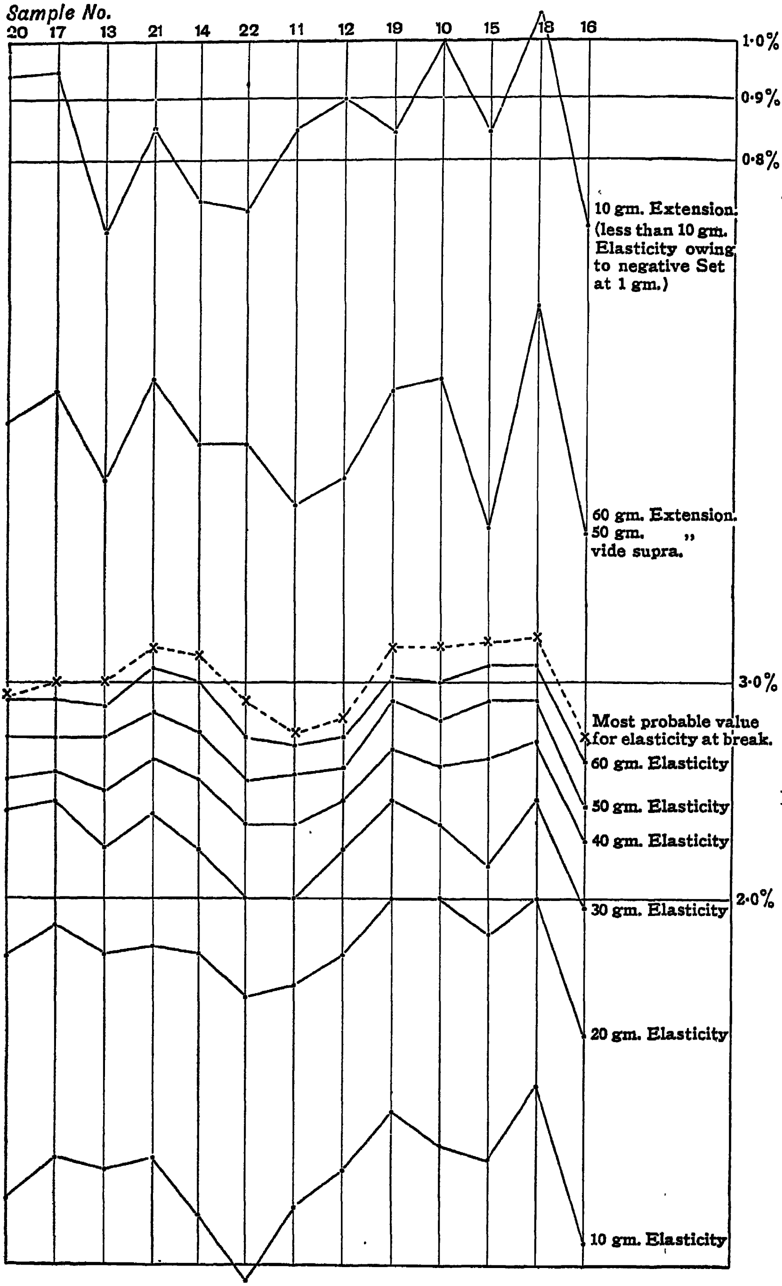


FIG. 97

98

99

100

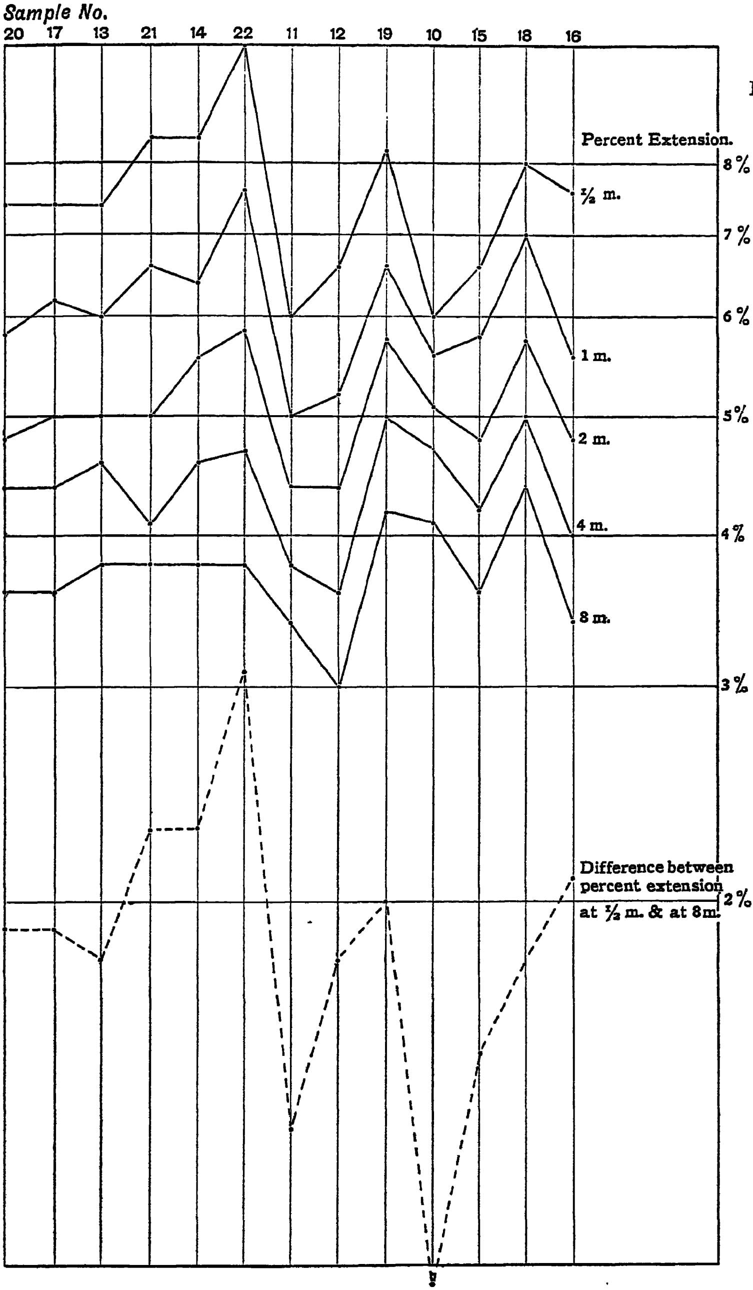

Sample No.
20 17 13 21 14 22 11 12 19 10 15 18 16
Percent Extension.
8 %
½ m.
7 %
6 %
1 m.
5 %
2 m.
4 m.
4 %
8 m.
3 %
Difference between percent extension at ½ m. & at 8 m.
2 %

FIG. 101

102

The true Elasticity is, of course, only "true" as defined for the standard test conditions of the Elastometer (pp. 189 and 351). It necessarily resembles the total extension, which includes it, and is shown in Figs. *99* and *100* for various loads. The differences from sample to sample are easily noted by comparing the relative positions of **Nos. 22** and **11** as the load increases.

The Permanent Set, again, as defined by the standard conditions of the test, is not plotted separately, no definite relationships having been detected for it.

That the total Extension at 10 grams in Fig. *97* is less than the Elasticity under the same load in Fig. *100* may at first sight seem wrong; it is the result of "negative permanent set" (p. 191).

These data are based upon one-metre lengths only. Another set of data for total Extension in various lengths is derived from the observations of Extension at Break, made with the Yarn Dynamometer (p. 367). These are shown in Fig. *101*. Incidentally, the discrepancy between the one-metre curve in this figure, and the curve which is nominally the same thing in Fig. *99*, may be noted as a good example of sampling errors and experimental difficulties. The sample-to-sample differences are very distinct; **No. 22** shows a sharp peak amongst its fellows at half-metre, which has faded away at the eight-metre length.

The outstanding feature of these last observations is the general phenomenon of the fall in percentage extension which results from increasing the length tested; at eight metres the relative extension is only half of that at the half-metre. This is a readjustment effect, and its sample-to-sample change can be seen in Fig. *102*, where the two extremes are **No. 22**, which changes from 10% to 4% as the length increases, and **No. 10**, which only changes from 6% to 4%.

For my present purpose it is sufficient to take note of the existence of these idiosyncrasies without attempting to elucidate them.

Strength Gradient and Peak Strength of the Yarns.—So far I have dealt descriptively with the principal facts available with respect to the yarns of the Thirteen Samples, and this simple descriptive method has been seen to be quite inadequate for the study of strength. It must therefore now be replaced by the analysis of strength into its components, the Gradient and the

Peak Strength, as defined in the previous chapter (pp. 213 and 216).

The renewed series of tests upon which this analysis is based was made under difficulties, due to very limited supplies of the original yarns. It was necessary to economise the number of tests applied, and to make the best use of the facts obtained in them. Consequently, the following analysis has no pretensions to great exactitude, though it is sufficient for its purpose. The reader should regard it as an illustrative example; not as a demonstration of the precision which it is possible to obtain.

As it is not very easy to discuss the whole of the Thirteen Samples simultaneously, even when the data and the methods are familiar, I shall first describe the method and results of the analysis for five of them only, after which it will be easier for the reader to recapitulate the process for them all.

A Preliminary Consideration of Five Samples

In the gradient diagram of Fig. *103* I have brought together the principal facts of this analysis for those five samples which gave the two worst leas, two of the three best leas, and one other. This diagram is not of the same kind as those which precede and follow it, although it also uses semi-logarithm plotting in order to show the gradient, for reasons given in the preceding chapter. Essentially it consists of five separate diagrams, which are superposed for mere convenience, each showing the various particulars for one sample; the neighbouring graphs show only one particular for all the samples.

If the reader will imagine that thirteen such diagrams as those in Fig. *103*, one for each sample, were drawn on transparent material and stood up behind one another in the usual order from **No. 20** to **No. 16**, it will be evident that the usual graphs which follow it can be regarded as the equivalent of sections taken through these thirteen along vertical planes corresponding to various yarn lengths. A preliminary inspection of these five will make it easier to visualise this later on.

Singles.—The five-point line which shows the gradient of each singles yarn between half-metre and eight metres is seen in Fig. *103* to point downwards to the neighbourhood of the lea value, except for **No. 16**; this falls away abnormally, and shows no definable gradient in these observations. More careful examination also

shows that none of the other four lines are as straight as they might be; this is the result of being restricted to ten tests per length per sample, in order to economise the limited amount of precious yarn remaining. Nevertheless the general trend of the

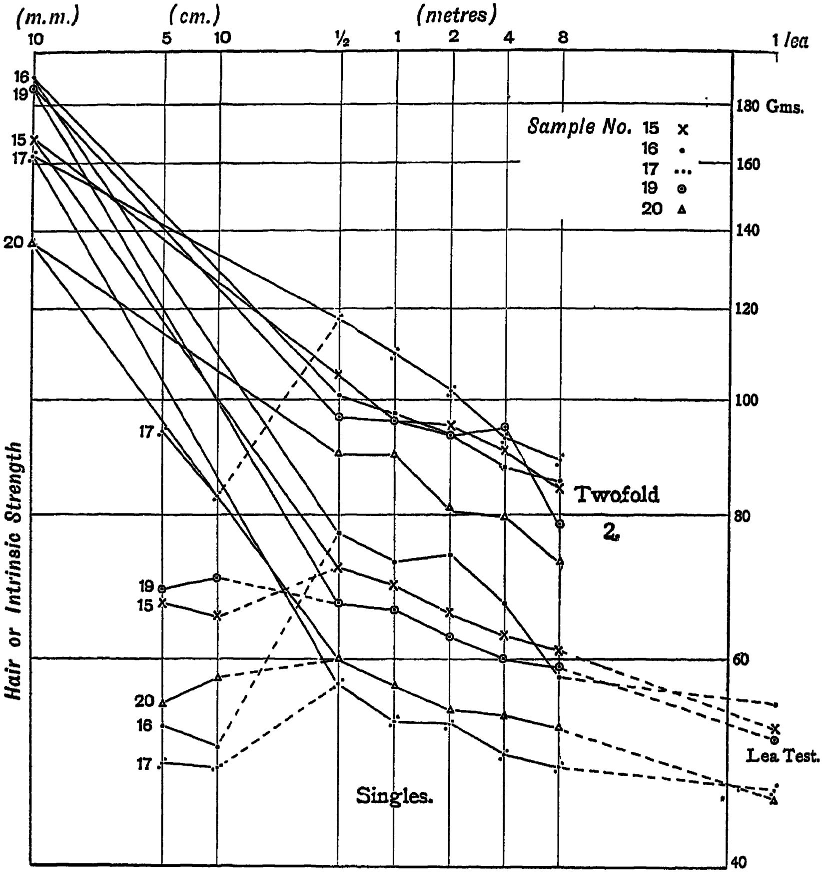


FIG. 103.

downward slope is clear enough, and serves to emphasise by contrast the behaviour of the same slope when followed upwards.

Following the gradient upwards, its direction is not towards the intrinsic strength (p. 217). Nor is it towards the short-length tests at 5 and 10 cm.; these are vitiated by the clamp-effect (p. 215), which makes them abnormally weak. If the

gradient is extrapolated to the 1 cm. ordinate, its intersection with this defines the peak strength (p. 216); this can only be done very roughly, but it shows the Peak to lie far below the Intrinsic. Thus **No. 20** has its Peak at about 70 grams as compared with an Intrinsic of 137 grams, or 51%. For **No. 19** the values are about 82 as against 187, or 44%. **No. 17** also has a Peak near 70 grams like **No. 20**, although its Intrinsic is higher at 163 grams, or 43%, while its lea test is also higher on account of a bigger Packing effect. Lastly, no extrapolation is possible with **No. 16**, whose "hump-backed" gradient I have deliberately included in these five samples, as a case which may be simple experimental error, or may be an extreme effect of the readjustment processes. The actual extrapolations are not figured.

The peak strength of these samples, though not very clearly defined, is only about half the intrinsic strength. This is a much bigger discrepancy than was shown by the good quality yarn cited in the previous chapter (Fig. 48), and is clearly due to these samples having been spun too fine. The point is important, because it makes their ultimate obedience to law and order all the more informative; there is an advantage in studying poor yarns (Chap. XV), in that they are less easy to explain, and the explanation is more comprehensive when it is at last attained.

The preceding chapter has shown that the gradient in singles is compounded between the weak-link distribution and the compensating effects of readjustment. It is not any guide to the efficiency of the drafting alone; the two-folds must be examined to estimate this.

Two-folds.—The construction of these from the original single, and their use in the weak-link analysis, was described in the previous chapter (p. 207).

The five-point gradients for these will be found in the upper half of Fig. *103*; the limitation to ten tests again makes these gradients none too straight. They are shown with the actual strength of the two-fold halved, so that the plotted strengths correspond to the strengths of singles yarn; the increase in strength is fully 50% over that of the same yarn previously tested as singles. The greater slope as compared with that of the singles is also evident; that the reverse should be the case if it were not for readjustment in singles has been noticed previously

(p. 208). The same clamp-effect is shown for the short-length tests, which for convenience are here plotted for **No. 17** only.

Instead of making a rough extrapolation of the five-point line to find the peak strength of the two-fold, I have joined the half-metre values directly to those of the intrinsic strengths. For **Nos. 17** and **20** the result is a steady, unbroken gradient; the Intrinsic and the Peak are identical; the strength at any length can be predicted if the gradient is defined. Yarn can have a "theoretical strength" after all (p. 201).

But this theoretical strength is still not attained by the other three samples, whose Peak is clearly less than Intrinsic, such as 130 grams instead of 190 for **No. 16**. This has still to be explained, and it should be remembered when seeking to do so that these two-folds were merely doubled to prevent readjustment, not to give maximum strength; thus it is perhaps rather more strange that this maximum should be attained by **Nos. 17** and **20** than that the other three should fall short of it. Indeed, these two are too perfect, for the small discrepancy due (p. 176) to resolved radial stress is concealed by the experimental error.

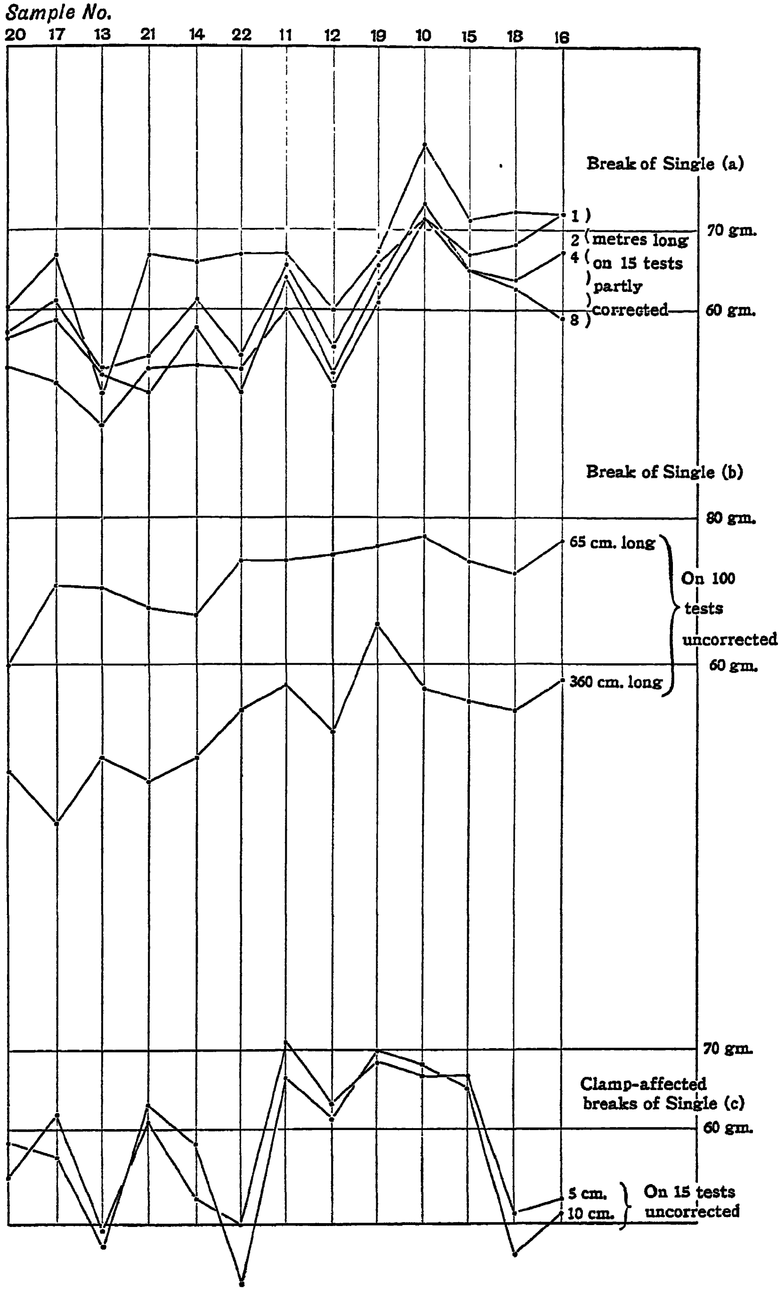
Returning now to notice what has been the actual effect of thus preventing readjustment; it has made a very marked discrimination in gradient between these five samples. **Nos. 20** and **17** have steep slopes; the other three have gentler ones, especially **No. 16**. But this is approaching a solution, for the two former are the two weakest leas, the others are strong leas, and **No. 16** is definitely the strongest lea of all. Reading off the inclination of the Gradient as an expression of the regularity of Drafting, this implies quite reasonably that weak leas are given by cottons which draft badly, strong leas by those which draft well.

Then again, the weak leas, or the bad drafters, were shown in Fig. *74* to have sticky hairs; that also fits in.

Also, these weak leas, being built from sticky hairs, show better hair-to-hair adhesion in consequence. Therefore they can more easily reach the yarn construction in which the hairs may be stressed to breaking-point before they will slip on one another; in other words, the construction in which Peak and Intrinsic coincide. Which is what has happened with **Nos. 20** and **17**.

Such an interpretation is at least a reasonable one. It remains

FIG.



104

105

106

to see whether it will hold good for the whole Thirteen Samples as well as for these five.

Final Analysis

Before proceeding to handle the whole thirteen, the reader might take note of the fact that this final analysis has to be made under rather severe limitations. Not only is the number of tests barely adequate for measurement of the gradient, but I have just shown that some of the samples still leave a gap between Peak and Intrinsic. The desirable procedure would have been to make further spinnings and doublings, but this was impossible. The available data have therefore to be used to show why this gap exists. The verification of fine shades of difference between sample and sample will not be possible. I have elsewhere described the work on these samples as being of necessity confined at first to simple observation; it is perhaps the more appropriate that they should drive me back to observation again, just when the right methods for experiment have become visible.

The chief need is a better determination of the gradient than by the guess-work fitting of an extrapolated line which I have so far employed for the five samples shown in Fig. *103*. That this method is impracticable when these scanty data are the only ones available can be seen from the simultaneous sections through the gradients of the singles in Fig. *104*. The two-folds are shown similarly in Fig. *109*.

In place of this I have used the method of halving-prediction formerly discussed (p. 205), which was shown in Fig. *59* for all the samples thrown together. It was there seen that this corresponds to reality in the two-folds only. I have, however, also used it to depict the " halving-change " in the singles also, where the predicted change is always smaller than the reality (p. 208); this plan gives a more definite estimation of the actual weak-link distribution at any given length, while still including the readjustment factor at that length.

The results of these computations of the gradient are shown in Figs. *114* and *113* for singles and two-fold respectively. The reader will notice that these two are mainly reciprocals of one another; a remarkable inversion, but one which is a necessary

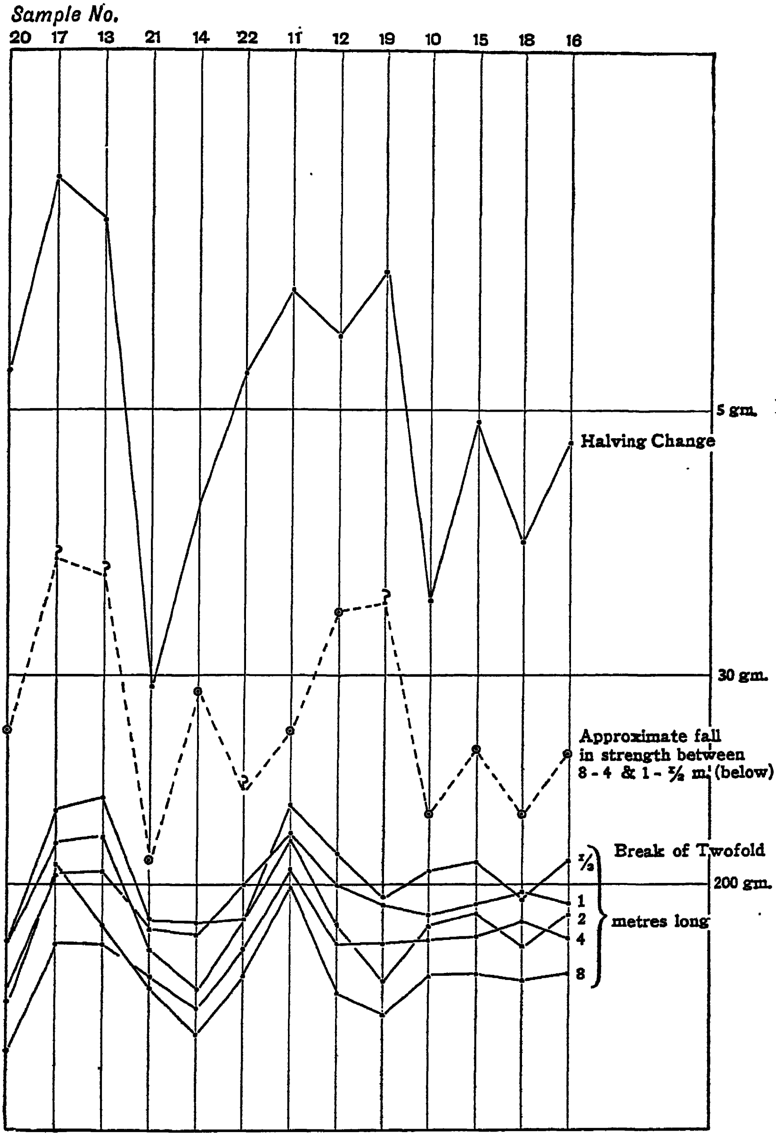


FIG. 107

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109

consequence of the compensating action of readjustment, over-emphasised as it is in these "over-spun" yarns.

Confining attention to the two-folds, their halving-change or gradient is redrawn in Fig. *107*, so that it may be compared with parallel estimations of the gradient made by fitting roughly a straight line to the five points; there is a good agreement, and the reader may be willing to take for granted that Fig. *107* is the more dependable. The gradients are here expressed in grams; to assist in visualising them among yarns of varying "strengths," they are shown again in Fig. *110* as percentages of the average two-fold "strength" depicted in Fig. *112*.

They are expressed on a definite basis in Fig. *111* as percentages of the intrinsic strength of each sample. They thus become comparable with one another, as gradients pure and simple. Their general trend is the reverse of the lea-test arrangement, the weak leas having steep gradients, the strong leas having gentler ones. This general trend is fortunately not true in detail; **No. 21** may be pointed out as a marked exception, for it drafts well (Fig. *111*) and yet makes a weak lea. But reference to Fig. *69* will show that this is because its intrinsic strength was very low.

Therefore the last step is to turn all the yarn-strength data into percentages of the intrinsic strength; Fig. *69* thus becomes a straight line at 100%. Under this is drawn Fig. *115*, showing the peak strength indicated by the particular two-folds which are being considered, *i.e.* full-twist singles doubled reverse way to give zero twist. No importance need be attached to the queer coincidence that two of them show a Peak which is exactly equal to the Intrinsic; the experimental data are not good enough for that; probably the real truth is that all the weak-lea end of Fig. *115* should be round about 95%, the margin of difference being due to stress resolved in radial compression (p. 176). But the fall away to the right is striking; the stronger the lea, the less nearly does the peak strength in this particular kind of two-fold manage to reach up towards the intrinsic strength. The only other curve of this type in the whole series is the one which shows the Hair Slipperiness (Fig. *74*), thus providing a measured instance of my second paradox, that cotton should be slippery during the drafting but sticky when twisted into yarn (p. xvii).

Comparison of this Fig. *115* with the gradients shown in Fig. *111* makes it clear that the more easily the two-fold's Peak

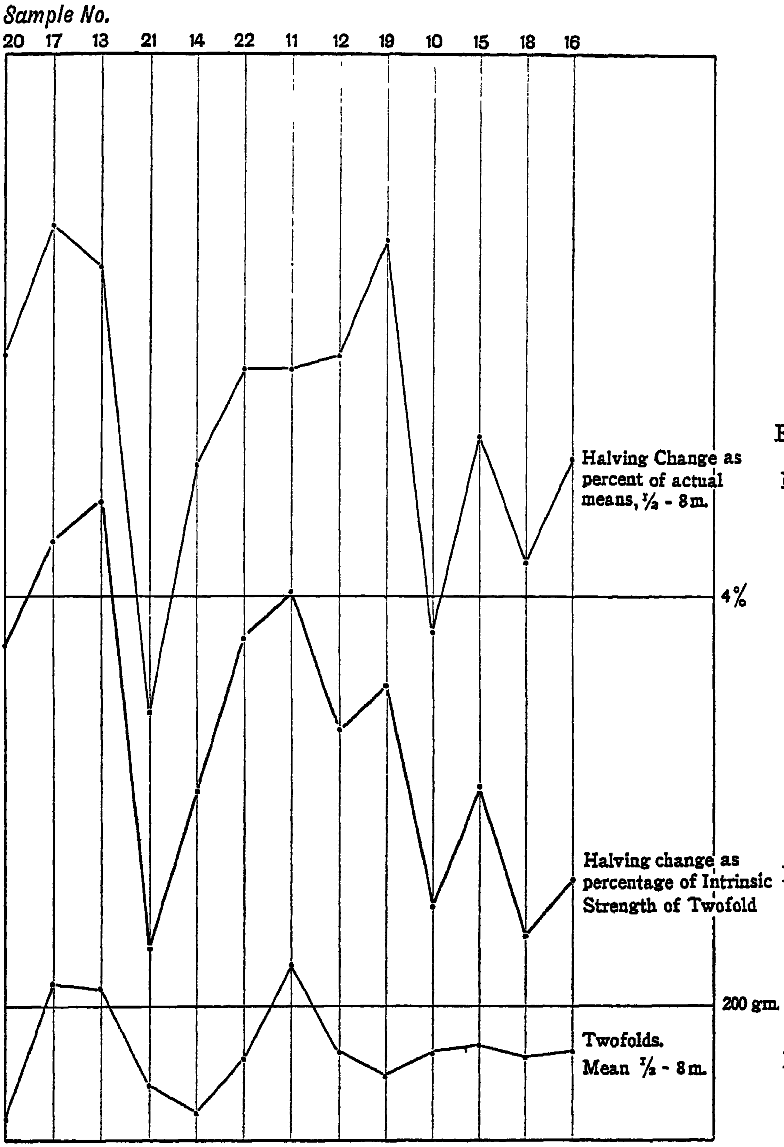

FIG. 110

111

112

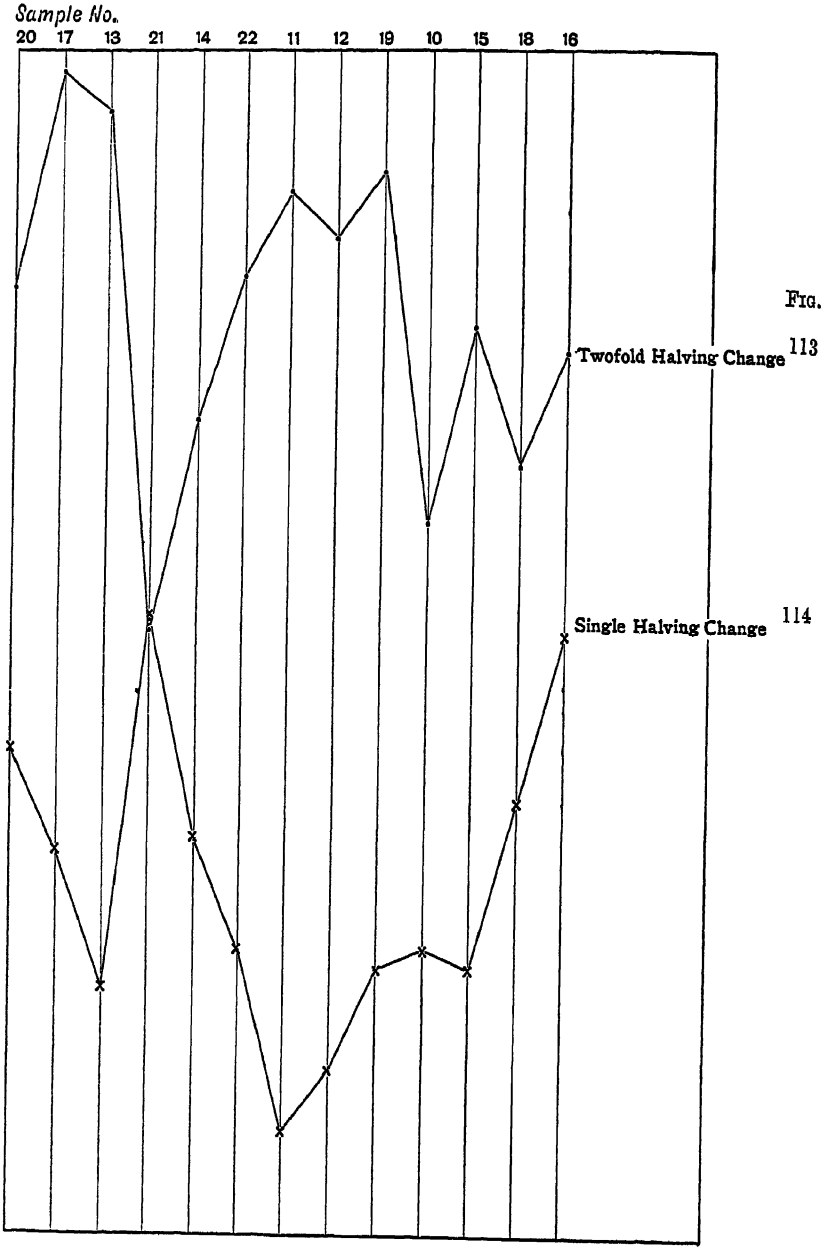
Sample No.
20 17 13 21 14 22 11 12 19 10 15 18 16
Twofold Halving Change
Single Halving Change

FIG. 113

114

can approach the Intrinsic strength, so also it has a steeper Gradient. Both are natural consequences of hair stickiness.

The average "strength" of these two-folds was shown in Fig. *112*; it is redrawn as a similar percentage of intrinsic strength in Fig. *117*. In the usual half-metre mill tests the left-hand end of the curve would be even higher, because of the steep gradients of the samples at that end; in long-length tests the curve would be flatter than in Fig. *117*. Combining this with the peak strength curve of Fig. *115* it becomes evident that each sample needs a different treatment in spinning and two-folding if its best performance is to be reached; but that performance will even then be limited (at any particular length tested) by the gradient down which it falls away from its peak strength.

The average singles "strength" of Fig. *81* is similarly shown in Fig. *118*. Here the effect of readjustment comes into play to abolish the general downward trend of Fig. *117*; the higher and lower percentages of intrinsic strength are scattered at random along the curve, and the rough estimate of peak strength in singles which is plotted as Fig. *116* is generally similar.

Much more computation and presentation could be made, even from the data already shown, but it does not seem necessary thus to over-work these scanty measurements; they have already demonstrated all the broad outline required. It will serve to remind the reader that the Yarn Gradient itself has still to be analysed quantitatively (p. 135), if I conclude this series of diagrams with Fig. *119*, which is like a window looking into the unknown. It summarises my search for periodicities of the drafting wave in these Thirteen Samples, made between the limits of 16 and 28 cm. only. Fig. *119* shows the periods which were found; there is no indication whether or not they were important periods, nor to what machine's series they belonged. All that can be seen is that they differed from one sample to another. It is definitely known that there were more of them outside the limits drawn in Fig. *119*, for one of the samples was searched from less than a millimetre to more than a metre, and showed many to be present, both longer and shorter than those recorded here. This is the unknown territory into which scientific exploration must extend, using physical experiment and statistical analysis in order that the Yarn Gradient may not merely be an observed and measured fact, however useful, but shall become a

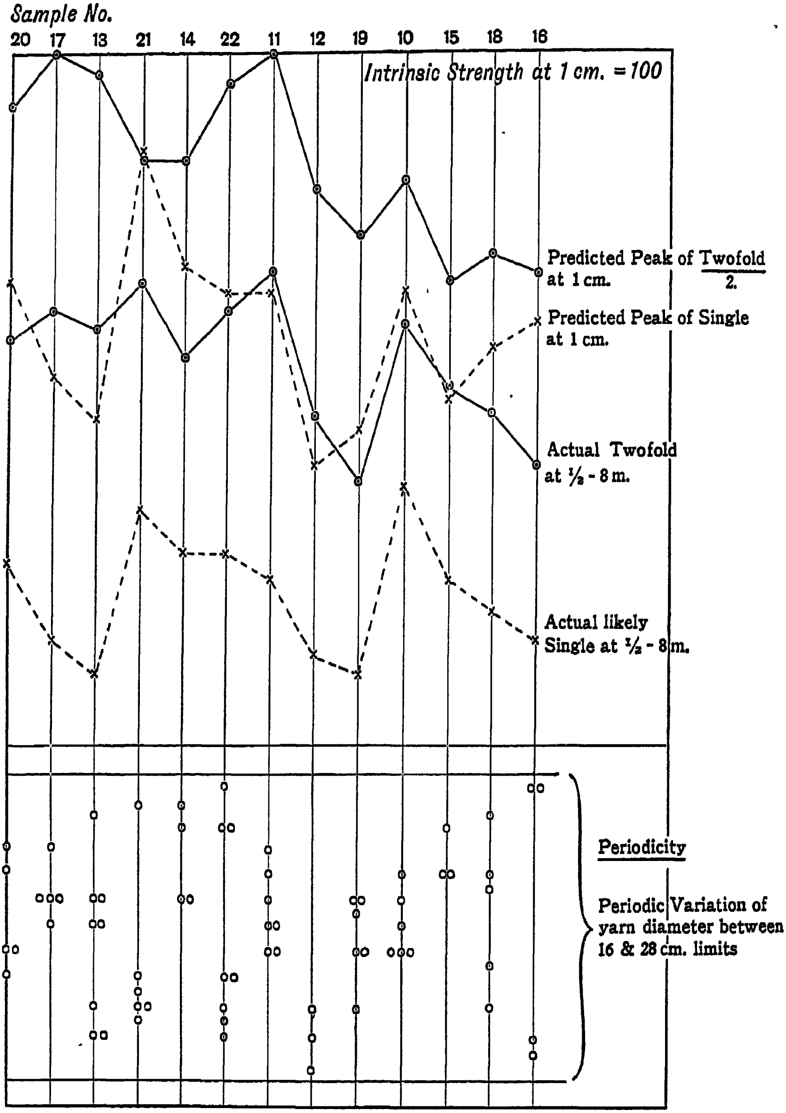

FIG.

(69)

115

116

117

118

119

predictable consequence of analysed drafting phenomena. Until this has been done, the analysis of "strength" in yarn cannot go very much beyond my present broad outline.

The major conclusions reached from this study of the Thirteen Samples are three in number. First, that the strength of the yarn is in every respect a logical consequence of three chief properties of the cotton hairs (cf. p. 199). Secondly, that these samples can be arranged in various orders of merit, according to the purpose for which they are to be used. Lastly, that while the lea test is a useful guide to their most likely and usual order, it is always composite, somewhat indefinite, and may sometimes be quite fallacious.

PART III

COTTON-GROWING FOR QUALITY

CHAPTER XIV

INTRODUCTORY

It may be premature to discuss the practical application of results which are so far from being exhaustive. In any case it is neither advisable nor necessary to attempt the task of relating them to the user's side of the Cotton Industry. Existing practice in the spinning mill is fully adequate to its present work, and will hardly be affected by more or less understanding of the reasons lying behind that practice, until they have been followed much further into detail than has here been possible. In the Appendix will be found some discussion of the methods which I have found to be employable for the effective prosecution of such industrial research, and the speculations of the present Part can therefore be restricted to the possible reactions which such studies as these may have upon cotton-growing.

The risk of prematurity in doing this is not so serious, because the further advance of knowledge concerning the problems of quality in cotton can best be made now from the grower's side. Although the indications about it which I have been enabled to detect are too sweeping for direct use in the mill, yet they do, I think, provide the main clues for which cotton-growers have been waiting, in order to set about the systematic control of quality in their crops (p. x).

The general position remaining at the end of Part II may be usefully summarised in terms both of ignorance and of knowledge, whereof the former is the greater, though expressible in fewer words.

The largest holes in the previous chapters which I can detect as examples of ignorance would seem to be as follows:—The statistical causation of the distribution of weak links produced by drafting is as yet unknown (pp. 135 and 203), even so far as those weak links are identical with low numbers of hairs in the cross-section of the yarn. Where weak links are modified in

singles yarn by readjustment (p. 172), the statistical problem is further complicated by physical and geometrical ones; it is possible to take refuge here from ignorance in the fact that insignificant use is ever made of singles in long lengths, outside the actual operation of weaving. The details of the connection between yarn gradient and hair properties are still much vaguer than I would like to see them (p. 266). The analysis of the effects of twist alterations upon the adhesion factor in yarn structure (Chap. XI) has been made largely by inference, in respect to the Thirteen Samples, in spite of observations on twist-yield and torsion. Why the length of the hair should be so insignificant a factor (pp. 156 and 252) may perhaps be included as an hiatus, not because there is any inherent improbability in the conclusions to which I have been driven with regard to it, but because those conclusions do not tally with the practice of the markets.

These and other gaps will doubtless be filled in before long, by knowledge acquired more quickly. The present stage has been reached so slowly that each line written within these covers represents more than an hour of study, without reckoning any assistance from my colleagues. Meanwhile, without forgetting the ignorance still to be rectified, the discussion may turn to such knowledge as is available for use :

(*a*) The properties of yarn which has been built in any specific twist construction, with the practicable skill now current in so doing, are dominated by, and in themselves express, the properties of the cotton used (Chap. XIII). Thus, in the last resort, the quality of yarn is in the hands of the Grower.

(*b*) Yarn is very far removed from the ideal cylindrical thread, with a gentle strength-gradient. Skill in spinning with the existing equipment (Chap. VIII) can only improve the gradient up to a degree which is fixed by the Grower, who also fixes the intrinsic strength (Chap. V).

(*c*) Fractionation processes may be developed which would carry out operations beyond the power of the Grower to effect (Chap. VIII).

(*d*) Lea tests for yarn (Chap. XII) and grading of raw cotton (Chap. I) are only dependable for comparison of similar

cottons for similar uses. They cannot be trusted for information as to the value or use of a new cotton.

(*e*) Different kinds of cotton differ more in the properties and possible uses of their yarn than they appear to do (Chap. XIII).

(*f*) Exact knowledge of cotton properties is becoming the best guide for predicting the industrial behaviour and value of that cotton in yarn or fabric.

In this Part my concern is principally with the control which the cotton-grower should be able to exert in virtue of this last clause. Its examination subdivides into three heads. First, the directive control which can be imposed artificially from the experiment station and its laboratories. Secondly, the partial escape from such control which results under the conditions of actual cultivation in the open field. Lastly, the long-distance objective of such control, over and above its immediate purpose of growing profitably for markets already extant. These provide the topics for the three chapters to follow. Some aspects of them have already been dealt with by two articles in the *Empire Cotton-Growing Review*,[1] and by one in the *Nineteenth Century*;[2] the present chapters can therefore be condensed accordingly.

[1] W. L. B., "The Perfect Control of Cotton-Seed," *loc. cit.*, 1926.
[2] W. L. B., "Cotton," *loc. cit.*, 1926.

CHAPTER XV

IN THE COTTON-GROWING LABORATORY

It has been shown by two entirely separate lines of investigation, on the one hand experimental (Chap. IX), on the other observational (Chap. XIII), that the length of cotton is only about the fourth character of economic importance which the cotton hair can possess. Intrinsic strength, fineness by itself, and the slipperiness of the hair, individually as well as collectively, have more effect than length as such in determining the properties of the yarn or fabric. These three are frequently associated with length (p. 149), but are not necessarily so associated, and should henceforth be given prior consideration in the selection and breeding of cotton plants for quality.

Thus to modify and extend the technique of applied genetics can only be effected by providing new methods for use in the cotton-breeder's laboratory, so that individual plants can be readily distinguished by measurements of their Intrinsic strength, their Fineness, and the Slipperiness of their hairs, as well as their Length. Much of this new technique has yet to be devised. There should also be studies in collaboration with the users of cotton, aiming at the simplification of spinning-tests. It will be convenient to consider these separately.

Breeding for Quality.—When the Sorter was re-designed as a testing appliance, I stated in the Handbook which was prepared to accompany it that " it is as yet a means for acquiring knowledge for its own sake, and is not yet a guide to the practical utility of any sample of cotton," and expressed my hope that when exact information had been obtained by both grower and spinner, " the study of the facts by many such students, from many view-points, will ultimately make such knowledge useful." [1] Although five years have elapsed, these quotations can still stand. The facts have been collected and studied meanwhile from both sides,

[1] *Loc. cit.*, p. 18.

but their relation to quality is still not very evident. No more need be said here; the method of measurement is available, even though its importance be quite subsidiary.

The use of the term "Fineness" is understood to imply its expression by weight, and the application of the hair-weight test (p. 69) in a genetics laboratory presents no difficulties whatever. The test is quick, convenient, and accurate, so that it could easily be standardised for routine selection work.

There is, however, a big environmental fluctuation in this character, which I have investigated in some unpublished studies made upon the bolls ripening from day to day which were formerly described in *Raw Cotton*.[1] Thus the cotton-breeder will be obliged to extend his measurements of hair weight by virtue of physiological considerations, as for so many other characters of the cotton plant. Indeed this large and easily measured fluctuation has already been used as an effective addition to the botanist's resources.

But hair weight has a further significance to the plant-breeder, wherein the available methodology is still imperfect, namely, in the variation from hair to hair. This is important industrially on account of its relation to "nep"; a compact frequency-distribution of hair weight enables a low-hair-weight cotton to be grown with less risk that the tail of the frequency curve will transgress into the zone where the hairs are too thin-walled to resist being rolled up into nep (p. 114). It is probable that there is a typical frequency-distribution, as for length, but the nep-free Yannovitch cotton, now extinct, shows that deviations from the type can similarly be found. I assume, and am corroborated by Dr. Harland's work,[2] that nep is almost entirely determined by this frequency distribution.

Now, there are no direct methods for weighing single hairs. Nor are the measurements of wall thickness dependable under the microscope (p. 65). Some method needs to be invented, and the most likely line would seem to be found in a suggestion which I owe to my former colleague in Egypt, the late Mr. F. Hughes; the rates of fall of hairs in a given liquid will vary with their weight and surface. If the latter is independent of the former the method should be workable, for the mean of the former is known, and the extremes, or the quartiles, could be assessed by

[1] *Loc. cit.*, Chap. IV. [2] Harland, S. C., *Jour. Text. Inst.*, 1924.

microscopic measurements of samples drawn from the upper and lower limits of the sinking precipitate of hairs. All hairs would have to be cut to constant length, and freed completely from internal air; these are only two of the precautions and difficulties, but Mr. Hughes' method is worth re-investigation in the light of the additional knowledge which has been gained since he first suggested it to me. No doubt some mechanical method for fractionation will arrive eventually (p. 163).

Turning next to the Intrinsic strength, as computed from the hair-break data in terms of a unit weight of hair (p. 67), there is much need for improvement in the methods of measuring the hair-break, and yet such improvement seems very difficult so long as dead-load testing is used; even with my magazine tester (p. 355) it is out of the question to attempt to measure more than a dozen or so of samples a day on each machine used, while the time taken to fill each magazine makes it impossible for one operator to see after more than two machines, even without computing the results. Such a method is of little use to the plant-breeder at the stage when there are hundreds of plants to be examined. Imagination fails to envisage a cotton-breeding laboratory equipped with twenty operators and forty testing machines, engaged in mass-production of data; it does not seem worth the boredom to the operators.

Some short cut is wanted for obtaining first approximations, which could afterwards be amplified by dead-load testing on the final selections. There may be a hint of correlation between break and ash-content in the data of the Thirteen Samples (p. 254); no doubt such correlations exist somewhere. But direct determinations are inevitable, and for these it should be worth while to re-investigate the impact testing (p. 228) of hairs, making the test in such a way as to measure also the extension under the impact; the resistance could then be evaluated. The design of apparatus to do this on centimetre lengths of hair presents various difficulties, as in the clamping of the hairs and in observing extensions which only amount to about a millimetre. Still, it seems to be the only way, and there can be no question of dispensing with measurements of the intrinsic strength when researches on cotton-growing are being conducted. The character is of first-rate importance in defining the upper limit of yarn strength, or rather, of fabric strength, and

its variation from one cotton to another has been shown to be greater than the world range of length. Methods must be found.

It would be possible to measure the Slipperiness character in a plant-breeding laboratory by the method described already (p. 70), making a weighed sliver in the draw-box provided with the Sledge Sorter (p. 58). This sliver could be twisted a definite number of times by hand, under a definite load, as it came from the rollers after the final drafting, instead of being collected on the folding roller. This would give a standard consolidation, and the untwisted roving test could then be made. But this would only serve in default of a better method; that described by Adderley,[1] in which the hairs are withdrawn from between two loaded pads of the same cotton, might be improved. Possibly the pads might be multiplied and set out on the magazine of the hair-break tester, but the need for rapid determinations rather suggests that the parallel between hair slipperiness and short-length impact tests (p. 230) should be examined further, using sliver instead of yarn. Various indirect measures of this kind seem possible, which would at least be comparable amongst themselves, but measurement in absolute measure is not yet in sight. The cotton-grower should be in a good position for experimenting in this direction.

I have confined this discussion to the four characters which are now definitely known to be of major importance (Chap. XIII). Others will no doubt transpire as knowledge advances further, but it would serve no useful purpose to deal with them more fully than the incidental references which have been already made in Part I.

Simplified Spinning Tests.—No keen research student of cotton-growing is content with the present situation, whereby his material has to pass through unknown hands and processes, and to be subjected to mysterious criteria, in order that its spinning quality may have a lea-test figure affixed to it. Many would no doubt be willing to attempt the making of their own first approximations to this measurement of quality, but this has been impossible in the past. It would seem, from the analysis given in the previous Part, that the function of a mill spinning test is primarily to evaluate the yarn Gradient obtainable under the conditions of everyday working. If this is so, it follows that the fact of small-

[1] Adderley, A., *Jour. Text. Inst.*, 1922.

scale spinnings on a laboratory bench being quite unable to simulate mill conditions is no longer an insuperable objection to them, for they could be made comparable *inter se*, and the various components of yarn properties could be compared separately.

The equipment might be reduced to one or two draw-boxes, like that of the Sorter, and a horizontal rule spindle (p. 104) of the throstle type. The relative gradients might be compared by cutting up and weighing a sliver, without even attempting to make yarn from it, as was described in an earlier chapter for the treatment of rovings (p. 127). The standardisation of humidity and temperature conditions within a small room or box is not difficult, and the investigations on roller settings and weightings which would naturally be made at the same time would help in analysing the drafting problem (Chap. VIII). Given a knowledge of relative Gradients, and of the Intrinsic strength, there remains the adhesion factor (Chap. XI); this can be as well investigated in coarse rovings as in yarn, by employing various twists, and testing for breaking load or impact strength, concurrently with the measurements of hair slipperiness.

It seems to me entirely practicable that the cotton-grower's laboratory should be able, if proceeding with scientific caution, to make useful first approximations to the spinning performance which his samples would ultimately show. Samples sent for proper spinning tests could then be accompanied by some indications concerning their virtues and vices as compared with samples which had previously been spun. The analysis of these actual spinnings might be extended further, on the lines described in this book, when cops of yarn were returned to the Grower from the Spinner, if the latter had not the time or the equipment for so doing.

Though such an expression of opinion can be no more than a personal one, yet to my mind it is very desirable that the grower should take up this line of research for himself, however trivial his attempts may seem to the professional spinner. I know from personal experience on both sides of the industry that a mere numbered sample of cotton which comes home for testing can but rarely be regarded with the same interest, even affection, as is given to that same sample by those who consigned it after sowing, cultivating, and even breeding it. Such a sample has its own

individuality to the grower; it is merely one of a series of victims when it reaches the spinning laboratory and mill.

Lastly, there would seem to be possibilities well worth exploration in the study of cotton under the conditions of hand-spinning. The same argument again applies; that some analysis into separate factors, and the making of relative comparisons for each of them separately, is now within sight of possibility. It would at least be interesting, if nothing more, to give little samples of ginned cotton, and also of prepared sliver, to several individual hand-spinners (who are usually available in cotton-growing countries), and afterwards to study their products and to listen to their comments. The average count obtained, as well as the gradients, should be informative. Realising now that the cotton itself dominates the situation, it would not be surprising to find a fair measure of agreement in the *relative* performances of various individual hand-spinners or spinsters, and of these again with the machines.

I have made these suggestions purposely casual. Some of them I have already explored very tentatively. They could doubtless be carried much further, if handled with care, and while so doing they would help to bridge over the gap in technique which still exists between Grower and Spinner. Their pursuit would give the student of cotton-growing a personal hold upon the subject of Quality, if not of Value, whereof at present he is completely deprived.

CHAPTER XVI

IN THE FIELDS

There are many features of everyday agricultural practice which can modify the quality of cotton, in the sense that they are capable of depreciating the quality available when the boll first opens. They have been dealt with by other writers, and need not be considered here in detail; it will suffice to quote examples. The crudest is the admixture of dirt and leaf while picking and handling the seed cotton. Prolonged exposure to "weathering" lowers the intrinsic strength by photo-chemical action, and by the attacks of bacteria and fungi. The surface properties of the hair are readily altered by the same causes, so modifying the hair slipperiness that the cotton seems fragile to the hands before the actual breaking load has dropped appreciably. Unsuitable ginning may break the hairs, or develop slip-spirals in them, or it may be unsuitable merely because it produces a bale with a texture which is inconvenient; there is room for much study of ginning, in spite of the negligible damage caused by good ginning practice.

My concern in this chapter is not with these secondary alterations, in spite of their practical importance, but with the still more important Primary Quality of the cotton which is available when the boll is ready for picking. The factors determining this primary quality fall into two groups, under the heads of that Heredity and Environment respectively, which have as their resultant the "average plant" whereof the crop is composed. It will be quite sufficient to limit the discussion to a few salient directions in which knowledge has extended since I wrote *Raw Cotton*.

Starting from the Heredity factors; it is too generally accepted that a control of seed supply is equivalent to a complete control of quality, though not of yield. Yet even a perfect seed control can only control the heredity factors, which dominate half the

situation; the environment is of equal importance. A super Sea Island cotton has grown coarse and wiry lint in the Sudan;[1] in other words, hair weight has a big environmental fluctuation. In the present imperfect state of our knowledge I am led to infer that the characteristics involved in primary quality which do not fluctuate much are: the true diameter of the hair cell, the frequency distribution of hair weight, the surface coefficient of friction, and the intrinsic strength of the hair. The available evidence is scanty for this last and most important character, but it is suggestive of such a specific peculiarity. Even if all these are practically constant they still leave several important components of quality open to environmental influences, and especially the mean hair weight, or fineness.

These limitations of quality-control by way of heredity are still further constricted by the facts of hair-to-hair variation upon any individual seed, already discussed (Chap. IV). It follows that no true uniformity of quality is even imaginable in the open boll. If it were eventually found to be desirable that all the hairs of a sample should be even moderately alike in their fineness, the cotton-grower could play only a small part in achieving this end. Indeed, when the fractionation of raw cotton becomes practicable (p. 163), the task of the grower might be simplified; however, if I am not too far wrong in thinking that intrinsic strength is at bottom a non-fluctuating and truly specific character, then, because no fractionation for strength as such is imaginable, the preparation of uniform fractions would only be possible when all the raw material came from the same kind of cotton. It follows that grower and spinner will presumably continue to assist each other. Whether fractionation at the spinner's end is better (p. 163), or purification at the grower's end,[2] or both together, will depend on the cost of production set against the advantages; a strict control of seed is costly to the Government of the country concerned, whereas the cost of fractionation would fall on individual spinners; it may be presumed that the former will be developed first.

The technique of purification of seed stocks, and of their distribution to the grower, can undoubtedly be carried much further, giving the seed-supply greater permanence from decade to decade. The organisation of distribution is especially import-

[1] *Loc. cit.*, p. 52. [2] *Loc. cit.*, p. 232.

ant, because a pure line stock of seed has no practical value unless it is fed to the grower through a system of seed renewal, to counterbalance the unavoidable contamination which any seed stock undergoes in field cultivation. The necessity for such renewal was first emphasised in *The Cotton Plant in Egypt* in 1912;[1] its employment has now been found worth while, even with semi-pure stocks, but the problem has yet to be tackled on the lines of civil engineering,[2] aiming at laboratory accuracy on a field scale of operation. So far as quality is dependent on seed supply it can be made invariable for an indefinitely long period.

The apparent superiority of quality in pure line crops, to which I directed attention in *Raw Cotton*, still seems to hold good, and has recently been noted by other students. The nature of this superiority is less obscure, now that the length character has been relegated to a minor position. Pure line crops may be almost as variable in their length as those from mixed commercial varieties, but the hair weight is much more affected by the "autogenous fluctuation" which results from constitutional physiological differences between one kind of plant and another; thus it follows that the gross variability of fineness should be appreciably diminished in pure line crops, thence improving the drafting and the strength gradient. Further, the pure line should show greater uniformity from hair to hair with respect to the intrinsic strength, and this also would assist in levelling up the stress-strain reactions of the hairs, so tending to make a more uniform distribution of stresses in the yarn when spun. Much analytical research is needed before exact discriminations can be made, in this causation of superior quality by mere "purity," but the fact seems now to be established, and the likely reasons for it are more evident.

The effects of Environment on quality can hardly be illustrated better at present than by citing the results from a series of spinning tests made upon the same selected stock of cotton, grown and ginned on the same estate for six years in succession, under the same personal control. Some small alterations in count have been eliminated by expressing the lea strength in terms of the "count × strength product" (p. 225), and this figure is given separately below for each sample tested. In some years the variety in question was used in experimental trials, besides being

[1] *Loc. cit.*, p. 178. [2] W. L. B., *Imp. Bot. Conf.*, 1924.

compared with other varieties in checkerboard plots; results from two different areas of land thus became available in the same season, as well as seasonal differences also. The results have not been analysed to show what share was taken in them by gradient or intrinsic strength, but some partial studies indicate the former as the major variable.

As an introduction to these data it will be useful to re-examine (p. 24) the shorter and simpler series for my four pure lines (p. 23).

CHANGES IN YARN "STRENGTH" BY HEREDITY AND ENVIRONMENT

Pure line.	1917.		1918.	1919.	All seasons for each pure line.
	Site A.	Site B.			
No. 77	2055	1665	1797	1800	1829
No. 95	2307	1828	2012	2074	2055
No. 111	1966	1515	1739	1667	1722
No. 310	2298	2053	2263	2457	2268
All pure lines for each season	2156	1765	1953	2000	—

So far as "quality" is measured by the lea test on singles—and it has been shown previously (p. 209) that the amplitudes of the real strength changes are greater than those of the lea-test figures—the changes due to environment on different sites in the same season average from 1,765 to 2,156, under equally good conditions of cultivation. This is nearly as large as the difference due to heredity in the average year between No. 111 at 1,722 and No. 310 at 2,268. Heredity and environment thus share equally in the determination of "quality" as thus measured. Though these differences are large, the real quality differences are still greater (p. 183); thus it is very certain that the worst yarn of No. 95 at 1,828 was really of better quality than the best of No. 111 at 1,966. The general dependability of the spinning test for any one year is shown by the constant relative positions of the four pure lines; when the figures given above for each one are expressed as percentages of the seasonal average the data appear as follows:

No. 77	95	94	92	90
No. 95	103	102	102	102
No. 111	91	86	89	83
No. 310	103	116	116	123

Returning now to the longer series of tests made all on the same variety, and thus illustrating the Environmental influence only, it should be noted that these were all good crops, probably among the best of their kind produced in each year. The following are the figures for each sample tested :

CHANGES IN YARN "STRENGTH" DUE TO ENVIRONMENT ONLY

Standard variety grown in—	Year.					
	1st	2nd	3rd	4th	5th	6th
Variety trials	2085	2426	2578	2225	2738	2462
	2024		2404	2214	2695	
	2147		2420	2222	2009	
			2416	1971		
Experiment plots	2024		2430	2813	2804	2615
	2147		2376	2862	2817	2536
			2420	2702	2793	2663
				2920	2599	2533
				2916	2606	2608
				2889	2655	2558
						2507

The average change from season to season ranges between limits of approximately 2,100 and 2,700. The changes from one site to another within one and the same season were just as great; 2,200 to 2,800 in the fourth year. The combined errors of the grower's experimental treatments together with that of the spinning test made a range of only 1,791 to 2,225 in the worst case. The general reliability of the spinning tests can be inferred from the fact that all the fourth and fifth year samples were spun at the same time.

The importance of environmental effects upon quality is very clear from these figures, even in this unanalysed form, and under conditions of cultivation which were exceptionally good. It is self-evident that the control of seed supply cannot do much towards standardising quality when the conditions of cultivation are less uniform than in these examples; as, for instance, when an erratic rainy season like that of Rhodesia is implicated.

It is also apparent that the broad facts which these lea-test data show would be much more informative when fully analysed into the separate constituents, which include intrinsic strength,

strength gradient, and packing effect. An attempt to write a book about such an involved subject as this yarn-cotton relationship has the advantage of revealing gaps in the research work which otherwise might continue to escape notice. In the preceding chapter I have indicated how the cotton-grower himself might proceed to fill up some of the many gaps left in my outline sketch.

CHAPTER XVII

IN COMPETITION

THE cotton industry has seen changes since 1914 which at that time were merely Utopian possibilities.[1] The joint inclusion of grower and spinner within one and the same great industry is an accepted method of dealing with its problems. It has a little experiment station at work in Trinidad on the study of fundamental problems presented by the cotton plant, as a plant, without reference to the local crop of any one country. It has at work for the industrial side another research organisation which, though still small in comparison with the enormous capital of that side, is a very large institution for this country. Its system of postgraduate studentships, held without obligation on either hand, is building up a corps of men who combine scientific qualifications with the farmer's outlook; this system has already had a beneficial effect on the status and standards of the over-seas agricultural services, besides serving the industry directly. Under the policy of developing native races instead of attempting direct exploitation, it has seen the production of cotton inside the British Empire increase most notably. It possesses a unique organisation in the Empire Cotton Corporation, which was so arranged with a triple weave of trustees, council, and executive that it cannot be dominated by the commercial and industrial interests, nor by the administrative ones at home or abroad, who are all equally concerned in its work.

These things are effecting such a change in the technical outlook of the industry as can only be appreciated by students of one side of it who had tried to get into touch with the other side before these things came about. They are, of course, only beginning, and have still to go much further, but it is already possible for the man from over-seas to learn more in a day than

[1] W. L. B., "A Central Department of Cotton Affairs," *Warrall's Textile Gazette*, Nov.–Dec., 1914.

was formerly possible in a month of sedulous gleaning. And again, the mill manager can be independent of Press stunts For the imagination, conviction, and driving force which have thus made reality out of notions, the cotton industry is primarily indebted to the late Mr. J. W. McConnel. His intellectual honesty enabled him to recognise the deep-seated ignorance of cotton which underlies the practical achievements of grower or spinner, and to realise that any steps towards knowledge would bring power, which could be applied.

Having been intimately associated with my lamented chief and friend during those five years in which he carried forward the main burden of these remarkable changes inside a highly conservative industry, I am sure that it will not be altogether waste of the reader's time to risk some speculation as to the future, knowing that Utopian ideas do sometimes materialise. This concluding chapter will therefore be devoted to a very light outline of the reasons which led me to refer, in the Introduction, to the "transition" through which the cotton industry seems to be passing.

These great alterations in the co-ordinated technical resources of the English cotton industry have an air of completeness which may prejudice their own usefulness. In the mind of their founder they were primarily devices for the reduction of ignorance; their use as means for the application of knowledge was definitely secondary, and a mere necessary consequence. No fully qualified expert on any branch of the cotton industry was conceivable to him. Our most notable and complete ignorance was in this matter of the yarn-cotton relationship, with which I have attempted to deal, but there were many others. In particular, there were three factors in the cotton-growing situation which have continued to develop further since his death, and now merit some attention. In the order of their respective importance they are: the increasing population of the world, the artificial textile filaments, and the development of knitted fabric. The last of these is too near everyday practice for useful discussion, but it is evident that the looped and springy structure of knitted fabrics requires different standards in its cotton and yarn from those which have served for weaving; roughly speaking, knitting demands fine cottons.

More discussion is possible concerning the other two factors.

The cotton industry is in sight of supplies which are more evenly distributed across the world, better controlled in their origin from seed, better classified to their most advantageous use, and better understood in that use. But the effects of world population and of artificial filaments may conceivably so act as to discount all these advantages, and to do so in a future which may be near enough to fall within the lifetime of some readers. They must eventually bring about a condition of competition, not merely within the industry between spinner and spinner, between grower and grower, or even between grower and spinner, but of the industry itself with other industries. All the technical resources already available, and very many more, will need to be worked to their limit in meeting this competition.

Consider first the World Population and its need for food. This subject has received attention in the Press as the result of an important address by Sir Daniel Hall [1] during my reconstruction of this book from its original form. Although estimates of the exact degree of urgency of the situation are necessarily discrepant, yet there can be no doubt that the increase of world population [2] will begin to utilise the whole of the limited areas of land which are available for food-production within the span of a generation or two. That is, of course, in default of deliberate control, working at least a generation ahead of events. The conclusions of Prof. E. M. East [3] would even estimate the "saturation point" to be due within a century of the present time.

A rough appreciation of the rate at which the available land is filling up can be obtained by recalling the boys' books of adventure written round about 1840. The virgin soils of the New World have disappeared under waves of wheat, so that Capt. Marryat's *Settlers in Canada* are as extinct as the dodo. There are no means available by which food can at present be produced except by starting from the activities of the green plant, which implies the necessity for suitable land upon which to cultivate the edible plant, to the extent of some two acres per head of population, or more; further, only a fraction of the land surface is capable of arable use, the rest being suitable for forest, for poor grazing, or for no agriculture at all. These are very

[1] Brit. Assoc., Sect. M, 1926.
[2] Pearl, Raymond, *Studies in Human Biology*, p. 584, etc., Baltimore, 1924.
[3] *Mankind at the Cross-roads*, New York, 1923.

elementary facts to the biologist, but I have not found them to be common knowledge otherwise.

Long before the saturation point of world population is reached there will be signs of its coming, whereof one of the first will be a rise in the index-number price of such crops as can only grow on arable land but cannot be eaten; cotton is such a crop. The production of food is inevitably a first call on human activity, however much it may be disguised in the life of cities; when land is scarce, then the use of land for any purpose other than food-production has to be stinted; food comes first, clothing a bad second. Native cultivators in simple communities grow their food crop in any case; if they grow cotton, they do so merely to provide cash, and while man may desire cash and its accessory civilisation, he has no choice about food. A very primitive community will clothe itself from the forests and wastes, not from its cleared patches of artificial arable land.

So it will follow, that when there is any appreciable approach to complete population of the world, the arable land will be reserved for food-production, out of sheer brute necessity; clothing will have to come from elsewhere, preferably from the forests which grow on land unapt for cultivation. A resort to animal clothing will not be possible, except from forest and waste-feeding animals, for a civilisation which was blind enough to starve itself would have no choice but to eliminate its animal husbandry. But the reaction between food and cotton will be evident long before the saturation point is attained, and will necessarily show itself first as a concentration upon quality. Poor quality cotton will be the first thrown out of cultivation; its place on the land will be taken by food-crops, its place in the mills by competing materials from the forests, waste lands, and waters. The better the quality, the more suited to specialised uses, the longer will it be worth while to reserve arable land for its growth.

In the meanwhile, lest the reader should think that because this may not happen in his time it is no matter, or because a world administration might conceivably step in to prevent things from going to extremes, still there is an intermediate stage which already shows signs of its existence. It is commonly claimed, and with good reason, that the introduction of cotton-growing into a native community has the effect of raising its standard of

living, through the influx of money for this cash crop. It is also common knowledge that the profitable cultivation of cotton depends eventually upon the availability of large unskilled labour forces for the picking season; the quantity of cotton which can be grown in a civilised country is sharply limited by this labour factor, and this limitation of quantity leads to concentration upon quality; Queensland is an example. But the raising of the standard of living by developing cotton-production is not endlessly compatible with the maintenance of large supplies of unskilled labour!

Thus the near future of cotton-growing is likely to be dependent upon the solving of the harvesting problem. The successful mechanical cotton-picker seems unlikely to appear, in view of increasing difficulties in the field, such as the close spacing of the plants which is demanded by the intensification of yield. A solution from the botanist is at least as likely as one from the engineer. It is also likely that more attention to the "primary quality" of cotton, as distinct from its dirtiness, coupled with improved methods devised in the mill to meet a changing situation, will enable the spinner to use cotton swept up from the plants by appliances which will collect it in a state quite unsuitable for the present machinery. I have previously (p. 19) cited an example to show that a dirty cotton can be a good one, and no opinion based on the present cost of waste-removal can be any guide to conduct in the possible future which I am here attempting to predict.

My second factor in this forecast is quite independent in its causation, but its operation happens to be intimately related to the food factor. The last decade has seen a very striking development in the commercial production of Artificial Filaments for textile use, especially those made by the regeneration of cellulose. How far it has already developed is common knowledge to all purchasers of clothing, but how far it has still to develop is mere speculation. Nevertheless there would seem to be several valid reasons for anticipating a development which will not be merely accessory to the cotton industry (which is the usual opinion), but will bring the two into direct competition.

One such reason may be found in the empirical basis on which the artificial silk industry at present rests. Not that its technique is not already highly specialised, and its control eminently scien-

tific; it is only permissible thus to describe it as empirical because its raw material, cellulose, is largely an unknown substance. The chemical nature and physical structure of cellulose, or, rather, of the different celluloses, are only now beginning to be unravelled. The tensile properties of existing celluloses are so little known that the fibrillar structure I have described for cotton (Chap. II) was a new discovery, while the important fact that the cellulose structure is the ultimate determinant of strength in yarn and fabric has been obscured from view by the old fallacious reasoning about "theoretical strength" (Chap. XII). Now, the process of "ageing" in the preparation of viscose solution for artificial silk spinning is of high importance to successful working, and it is largely a mystery. If it be assumed, as is quite permissible, that the tensile strength depends primarily upon inter-atomic adhesion, then the problem of strength in artificial silk is the problem of building molecular chains in the ageing process, and then arranging these chains within the filament in a manner quite analogous to the way in which the cotton-spinner arranges hairs in the yarn, or to the way in which the fibrils are arranged by growth-processes within the hairs. If this is so, then the present-day artificial silk is as much inferior to its future successor as the tangled "condenser yarn" of the cotton-waste spinner is to the drafted yarns which this book has discussed.

It is also a fair justification for expecting such improvements in the quality of these artificial filaments to look back on its history from the days when Réaumur, nearly two centuries ago, tried to imitate spiders' webs with gum. Chardonnet's silk of the 'eighties had beauty, but at first it was highly inflammable. Non-inflammable filaments from the cuprammonium process, and the early viscose filaments, were again regarded with tolerant interest because of their loss of strength on wetting. The last-named process was patented by Cross and Bevan nearly a quarter-century ago, but it took years to develop a satisfactory technique for working it, so that even in 1914 I could write:—"Detailed objections . . . are all irrelevant, for they will all be overcome in the laboratory, and the results applied in competition with cotton, when they pay. Such conjectures may be considered as looking too far ahead for practical men, but their general principle is unassailable, namely, that a specialised source of supply cannot

compete with a generalised one, if other things are equal. Whether the other things can be made equal, by research, remains to be seen, but it is not wise to deny the possibility." [1] The intervening decade has seen this equalisation well advanced.

Nor is it unreasonable to anticipate a direct price competition between these filaments and cotton. Taking an approximate comparison of yarns ready for similar use in the knitting machine, there would seem to be a present advantage of about 30% in price on the side of cotton. This is not a large difference when direct competition is as yet scarcely noticeable, and there seems good reason to believe on more grounds than one—chemical, physical, commercial, as well as agricultural—that the price advantage might eventually be shifted to the other side. Such special products as the silks which contain a large weight of the acetyl group must, of course, remain much more costly.

If equal price followed without any improvement in the present quality of artificial silk, the position would still be very serious for inferior cotton. Detailed citation of intrinsic strength and strength gradients would be needed to establish the comparison clearly, but the reader may perhaps accept a general statement that the good artificial silks of to-day are already better than low quality cotton in some comparable yarns. The sole advantage which cotton as a whole possesses over these empirical products is in its strength, especially when wet, and that advantage is being lessened.

Cotton yarn has no advantage in being a yarn instead of being built up of continuous filaments. The observations on the Elastometer, formerly cited (p. 189), may be used again in illustration of this. The yarn spun from short lengths of artificial silk is able to display the defects of yarn structure, as well as the defects of artificial silk, and the advantages of neither. This is being learned by the housewife, in the comparative freedom of artificial silks from the creasing-defect of cotton.

But my chief reason for anticipating that cotton will meet with serious competition from the artificial filaments is again in relation to the food supply. The viscose process utilises spruce wood, though it could utilise other celluloses from the forest areas; in other words, its supply of raw material takes no toll from the shrinking areas of arable land which as yet are

[1] *Loc. cit.*, p. 244.

not cultivated. The significance of this eventual advantage over cotton has already been made out (p. 301). It is true that clothing is by no means the only demand made by the world population on the forest areas; at present it is only a very minor demand, and the conservation of the world's timber is already a serious problem, accentuated by the demands of the daily Press for paper. But I have expressed an opinion in favour of further improvements in the manufacture of artificial silk as the result of research into the constitution of the different celluloses. This would widen the sources of supply; for cellulose, in one form or another, is a plant product; only two forms are at present convenient for this manufacture, spruce wood and cotton linters. But the available supply of other forms and kinds of cellulose is only limited by the supply of the higher plants which is available in the whole world. Eventual decisions as to which of these may be used will depend on the cost of production, and there is no inherent reason why further knowledge should not make it possible to clear up such waste-heaps of the world as the Nile sudd area for the making of clothing, if it paid to do so.

The position of artificial silk is thus analogous to that of power alcohol, as that of cotton is to petroleum spirit. Resort to some such fuel as alcohol is inevitable in the long run, if the internal combustion engine remains an important source of portable power, because its supply is only limited by the supply of plants, not to mention that synthetic alcohol is available as well. But the analogy goes further, for at the present time it is already economically impossible to sacrifice edible crops from arable land for making power alcohol; sugar-beet, maize, or potatoes are too costly; poor-land crops like the Jerusalem artichoke, or waste material of forest origin, are the highest-grade sources available.[1] Cotton is wanted more than power alcohol at present, but there is a strong probability that it will also be driven off the arable land eventually, except in so far as it can show advantages over the artificial filaments.

Thus the problem of cotton supply in the Future returns continually to a condition of justification by quality. Even at the present day, and in spite of the apparent contradiction afforded by the limited market for such cotton as the Sea Island from the West Indies, there are indications of a continuing and

[1] Brit. Govt. Report, 1922.

increasing insistence upon quality. The history of my No. 310 stock in Egypt is a minor case in point. The instinct of the plant-breeder has always led him to attempt the improvement of quality, even though quality was an indefinable quantity, and although the markets insisted that no improvement was needed, but merely a maintenance of the best already extant. Still, the unwanted kind of improvement which M. Sakellaridis effected by his production of Sakel cotton was not rejected once it had materialised. It is unfortunately true that the grower is at present afraid to grow good cotton, because quantity is saleable, while quality may not be; the change in marketing arrangements which has so long been desired may yet come automatically by way of a demand for quality to meet competition from the artificial products, and by way of a refusal to use food-producing land for any crop which is not valuable.

A cotton industry which thus concentrated upon quality, both on the spindle and under the hoe, would necessarily be different from its present form. The nature of the probable change may be summarised as a change towards a "structural" use of cotton in place of a "covering" use. Cotton has advantages of strength combined with flexibility for such structural purposes as the pneumatic tyre, semi-flexible couplings, belting, or type-writer ribbons, in which it should be able to hold its own. These engineering uses for it are likely to increase as its advantages become apparent in such matters as the neglect of lubrication, quietness, and durability; there has been a steady increase in this substitution of cotton for metal in the case of the automobile. It is in this direction alone that cotton can be practically free from competition, and it is there that my admittedly narrow interpretation (p. x) of "quality" as being primarily identical with flexible strength is entirely justifiable.

In conclusion I would quote from a recent publication of the International Institute of Agriculture [1] for a clear statement of the position in which the cotton-grower has until now been placed.

> "Quality, in cotton, is a very vague term, and cannot be conveniently or tersely expressed, except in terms of the market price."

[1] *The Cotton-growing Countries, Present and Potential*, London, 1926, p. xxii.

It is my hope that the reader now finds the term somewhat less "vague," and that although its partial definition in this book can by no means be described as "terse," yet my attempt should serve to point out the roads by which precise definitions of the components of Quality can be reached. I have relegated to the Appendix an account of my own views concerning the best methods for travelling on those roads. The traffic on them will soon be congested, for although my own researches were of necessity too independent (p. 347), yet I am well aware that parallelism of ideas is a very common event when industrial research is active. Thus the desirability of knowledge about the meaning of Quality in cotton went unheeded for a century or so. Our ignorance became evident by the time that Mr. J. A. Todd first led me with him to look for information in Lancashire during our leaves from Egypt, some twenty years ago. Now the problem of Quality has become one of general interest, and the situation has meanwhile changed so quickly that the complete solution of that problem is no longer desirable; it has become an imperative necessity. This necessity is not merely one for the scientific worker, but for the industrialist himself, and for his operative staff. The cotton industry of the future, perhaps smaller, certainly more specialised than at present, will stand—or may fall—by the quality of its product.

APPENDIX

SOME METHODS OF INDUSTRIAL RESEARCH

INTRODUCTORY

The appendix of a book is usually employed to store such material as is relevant to the main text, though too highly specialised in its interest for inclusion therein. To that extent the present Appendix, in dealing thus with the specialised technique of research work, is appropriate. It is, however, somewhat anomalous in its treatment of this material, which is arranged as a supplement to the main text.

This arrangement was developed as the least unsatisfactory method of reconstructing the pre-existing text (p. 5), wherein several aspects of research work were discussed as they arose. Some of that discussion has seemed to be worth preserving, being itself a summary of several papers which I have read under varied circumstances during a period of several years. Yet there is no justification for attempting to make a separate book on the subject, because any use which these personal opinions may possess is conferred upon them, and is illustrated by, the contents of the rest of this volume. For the same reason it would be waste of paper to make the available material into articles for technical journals or the reviews, thus entailing a page of prolix and technical explanation to every paragraph of substance. Another, and more practicable, course would have been to leave the material within these covers, where it properly belongs by right of birth, but to bifurcate the text at the end of the Second Part, writing two separate Third Parts; one for the cotton Grower, the other for the cotton Spinner. The former alternative has been done, but to do the same thing in a strictly parallel treatment for the spinner would have demanded a much more dogmatic attitude about cotton-spinning than I have any right to assume; whereas, by restricting my treatment to the technique of research in cotton-spinning, I am less likely to be seriously in error. The present Appendix is thus an hypertrophy undergone by one portion only of such an alternative Third Part.

The fact of thus restricting the treatment in one direction makes it widen out in the other direction, for research work is

governed by the same considerations, whether it be done in a cotton-mill, a cotton-field, an aeroplane, or in a buried city of long ago. This Appendix, therefore, begins broadly, and narrows down as it progresses, till it concludes with such ephemeral details [1] as the particular " gadgets " devised for gathering the data used in the main text.

The reader will no doubt have detected the undercurrent of interest in Method for its own sake, which runs through that text, in spite of my deliberate relegation of many details to this Appendix, as formerly mentioned in the Introduction. This matter of Method, in its widest sense, is, to my mind, the prime factor of industrial research. Circumstances happen to have given me the experience involved in not merely starting and developing an industrial research organisation, but in making simultaneously for it a " science " in which it could exercise its powers (p. xvi). My raw material was an empty room, some scattered proofs that spinning results sometimes disagreed with expert judgment (Chap. I), a scepticism engendered by hyper-critical search through the literature then existing, and Mr. McConnel's interest. The provision of methods for obtaining knowledge was thus of paramount importance throughout the whole ten years, whether those methods dealt with the strongest way of knotting a yarn, with the form of a cam for the sorter, with the graphing of data, with the training of an assistant's interest, or with the best use to be made of an available cellar. Methods had to be found for translating practice into theory, and for the testing of theory in practice; for reconciling the international exchange of scientific knowledge with business privacy; for stimulating co-operation and individualism simultaneously within the staff; for diffusing new knowledge to make it of use, without letting it diffuse too far, and without boring the recipients. Lastly, a method which would ensure that the work so started would continue automatically.

The variety of interests thus lightly handled, together with the fact that my methods had to be built to do unknown work, and yet to do that work in the act of building, must be my authority for the form and substance of this Appendix.

[1] Though a quotation from Leonardo da Vinci is irresistible :—" Mechanics is the paradise of the mathematical sciences, because only in mechanics is it possible to taste the delightfulness of their fruits."—*Pensieri della Scienza*, LII. *Della mechanica*, Barbera, 1899.

Section I

DIFFERENT KINDS OF RESEARCH [1]

IT is not always easy to explain the purposes of the research worker to that great majority of people whose only contact with any form of research has taken place through some such eventual application thereof as a dynamo or a vacuum flask. The difficulty is very evident under the circumstances which attend on the formation of an industrial research association. Not only is there difficulty in making explanations to constituents about the things which it is hoped to do, when those things are themselves inevitably unknown, but even the organisers are liable to talk at cross purposes. Those who embark on research in the spirit of a plain raid for loot can be neglected, but apart from them it is usually desired that the organisation shall not miss its chance of achieving big results by fixing its attention on things which are too trivially practical to involve big issues; the telescope should be used alternately with the microscope. But there is no very clear terminology in which such discussion of policy can be conducted; one person advocates "fundamental" research, another condemns it and demands "applied" research, although both may mean the same thing.

Simple distinctions between "deep-digging" and "trouble-curing" can be made and understood, but they do not go very far in sorting out the complex forms of technique and organisation which the research worker has to use and build. Further subdivision in order to effect such classification brings about increasing ambiguity in the meaning of the terms employed; such sequences as those in the following list have been used:

> Pure research; fundamental research; applied research; investigation; invention; routine.

It will be seen presently that these names correspond to genuine differences in the type of activity which each one attempts

[1] A summary of this section has appeared in *Nature*, August 26, 1926.

to describe, but their connotation is only clear by mutual agreement.

The act of organising such activities for industrial purposes, when the crafts of the laboratory, of the workshops, and of manufacturing plant have all to be brought into co-operation within an organisation, presently reveals the fact that in any of these varied forms of research there are similar subdivisions of the work done. This subdivision depends on the amount of restriction applied to it, or conversely, on the degree of freedom. The mechanical analogy of the "degrees of freedom" in a three-point support may serve for illustration.

The three prime essentials of research work, in which it may be free or restricted, can be denoted as the Method, the Subject, and the Aim. The research worker may be left free, either by natural circumstance or by artificial organisation, to pursue his own course in one or more of these; or he may be restricted to a limited choice amongst their possibilities. With the two alternatives of freedom and restriction in each of these three essentials, there should necessarily be eight classes of research work. Further subdivision might be effected by defining the degree of restriction, if any, but this is needless at present. The eight classes can be denoted in writing by the use of capitals and small letters in combination, using capitals for a free condition, small letters for a restricted condition; if the Greek mu, alpha, and sigma are used for these small letters, the description can be made verbal. Thus "M.A.s." would imply freedom in method and aim, with a restriction to some particular subject, such as cotton. The eight classes are, of course, as follows:

M.A.S. M.A.s. M.a.S. M.a.s. m.A.S. m.a.S. m.A.s. m.a.s.

It should be understood that the term "method" is used for this classification in a restricted sense, to imply experimental method, though even so it covers a wide range and includes statistical and mathematical methods for the study of data already obtained, as well as the methods used for obtaining them. The activities of all these classes are necessarily conditioned by mental method, to a greater or less extent. This "super-method" involved in the amount of thought applied to their results may vary in its importance from a maximum with the pure mathematician, like Descartes, to a minimum in routine

assistance. Again, the methods of direct experiment may be temporarily or permanently inapplicable to a problem, as in much biological or geological study; a quotation from Vasiliev's discussion of the Greek philosophers will serve. "Aristotle gives enormous importance to the processes of induction and classification; and his philosophy may be correctly called a philosophy of a biological type." [1] In many industrial problems this form of mental method is the only one available during the early stages of study, but whereas the geologist is almost bound to it, the industrialist can almost always escape from it by diligent search for direct experiments.

Returning to the narrower considerations involved in the "Method-Aim-Subject" classification, it is worth while to notice the inter-relation of these three before applying it to actual cases. The fundamental relation of experiment to the advancement of natural knowledge, as observed by Francis Bacon, evidently sets the Method in a position of superiority over the other two. Consequently, such research as is done with its methods under restriction is likely to be of a low order; the lowest order is "m.a.s.," typified by the works-control laboratory, or the routine of a testing-room. On the other hand is the university laboratory, which is potentially unrestricted—finance excepted—in all three degrees; it can use Riemann's geometry or glass-blowing, can study chromosomes or nebulæ, and need take its aim no closer than at the advancement of human learning. The testing-room is not only restricted to quick routine methods, both in the making of its tests and in the statistical treatment of its results, but its subject-matter is restricted to the materials made or used in its own factory, and its aim is equally restricted to narrow and well-defined ends. The Aim is of rather higher status than the Subject, in the sense that it is eventually immaterial whether botanical knowledge be acquired on sweet peas and applied subsequently to cotton, or whether it be obtained from the cotton directly.

Some instances of the application of this classification can now be taken from the main text, beginning with the four groups in which method is free.

M.A.S. From the nature of my restricted subject it can

[1] Vasiliev, A. V., *Space, Time, Motion*, London, 1924, p. 18.

hardly provide any good example of this, the university type of "pure research." Perhaps the nearest approach to an example is provided by the observation of "trapped twist" in the doubling frame, simply because the original observation was entirely spontaneous and accidental. The discovery had no sooner been made than its class was changed.

M.A.s. This class is commonly neglected, but it corresponds to the "fundamental research" which is advocated by broad-minded industrialists, and on it only can the whole structure of industrial research be securely builded. It is pure research in every way, except for the restriction to a single subject. Simply as an exercise in the craft of research it is valuable, in that a difficulty encountered cannot be dodged by changing to "less unsuitable material"; some way round, or through, or over must be found, or be left for time to provide. My personal experience of this class has been chiefly on the botanical side, but much of the work described in this book concerning the microscopic structure of the hair belongs to it (Chap. II); the discovery of growth-rings and fibrils started in this class, though the way in which their study changed to the restricted aim of explaining strength in yarn is apparent in the later pages. The creed of the research worker in this class may be condensed to an assertion that any knowledge about a useful thing must eventually itself be useful.

M.a.S. The restricted aim implied by attempts to elucidate the nature of the strength Gradient (Chap. XII) was taken without any restriction on the methods employed, and with very little restriction as to the subject. Observation on yarns other than cotton, including continuous-filament structures as well as spun yarn, on rubber and wires, and miscellaneous information about wire ropes, was more or less involved, though only secondarily. The methods ranged from the invention of the yarn dynamometer to the statistical assistance of Mr. Yule. An example from outside is provided by the medical profession; most of the work done for public health has one definite aim, with a great variety of disease-provoking subjects, and all kinds of methods for their study.

M.a.s. The present volume belongs to this class. Its aim is the cross-connection between raw material and yarn, its subject is cotton. Both aim and subject are narrowly defined and

restricted, but this does not prevent its methods from displaying a certain variety. It is typical of the everyday research of most industrial laboratories, which deal with specified materials or processes, and have to take their aim at effecting paying improvements therein.

The next three classes are all restricted in their methods, and the possession of freedom in the minor essentials only will be seen to have altered the type of work to a lower grade. Human nature does not willingly continue to work at some method without speculating as to the aim of its work, nor does it devise a method without looking round for a subject to victimise thereby. Hence these three classes may be termed the " unstable classes," and in them true research merges into technicality, love of detail, copying, or " gadgeteering."

m.A.S. The position of the Harp periodoscope at the time of writing may serve for illustration, while it is waiting to be re-designed in more convenient form. It was devised during M.a.s. work on the irregularity of yarn diameter, and when available for use it was applied as pure routine in m.a.s. measurements thereof. As the results accumulated it became evident that the aim should be taken at something lying deeper than simple periodicity of diameter, and I have shown (Chap. VIII) that this aim is still vague, so that the work has changed to m.A.s. Meanwhile the apparatus has been found to have possibilities for the general purposes of computation work, and so has passed into the m.A.S. class. In general, this class represents the kind of research which is carried out in order to investigate the usableness of a new discovery, such as Röntgen's rays.

m.A.s. With the sorter representing the restricted method, and cotton the restricted subject, the Handbook of that instrument may be taken as an example of this class. Facts could be gathered by its means about the length of cotton hairs, but the aim to which the collection of those facts could be directed was quite obscure, as has already been noted. It is obviously a temporary class, through which all research work passes at some stage of its development, and might be described as a phase of technical benevolence.

m.a.S. The Rule spinner (Chap. VII) or the super-draft appliances (Chap. VIII) fall into this group, being essentially

applicable to any textile material, though the aim is restricted to reducing nip-ratching or to minimising the number of drafting operations. The majority of patentable inventions may be ranked here, as being particular ways of achieving something, though applicable to many subjects; the "rip-fastener" is a case in point.

Lastly comes the class of routine work, which is again clear-cut in its triple restriction:

m.a.s. All the routine work of the testing laboratory by which the data for yarn strength in this book were obtained, the working of the mill machinery by its operatives, and repetition construction in the workshops, are here included. According to the degree of interest, apart from manual skill, which is shown in the conduct of this class of work, so it is likely to give rise to an improvement in method, to perceive a new aim, or to find application to a new subject, and thus pass up to a higher class. New recruits to the research staff proper may thus be found. An example of the class which is nearly perfect in its narrow restrictions of opportunity can be taken from the untwisted-roving test (Chap. V).

I have found that the analysis of activities embodied in the foregoing classification is helpful for the administration of research work. For example, it defines the need for additional junior assistants, and detects opportunities of promotion for them. Meanwhile it will serve to illustrate the infinite variety of industrial research, which always includes those five classes wherein the Subject is restricted, and often overflows temporarily into the remaining three.

Section II

THE RESEARCHER'S CODE [1]

The work of the pure science laboratory is quite peculiar in one respect; it has no secrecies. Researches in progress are discussed freely; the effort of the worker is directed to disseminating knowledge; all his technique is devised with the object of making sure that nothing is thus spread abroad which is not the best possible approximation to the truth. This custom is invaluable as an automatic check on bad work, since all published results are open to criticism, to approval or to refutation, by anybody of sufficient competence. To be secretive in a research laboratory is very bad form.

This peculiarity of scientific work has a bearing on social problems. The merits and demerits of Individualism and of Communism are usually discussed as mutual incompatibles. But the scientific worker may claim to have reconciled the two incompatibles by developing a code of conduct—it may be called the Researcher's Code—which demands the strictest individualism in the effort, and in any credit, followed by complete communism in the results of that effort.

This Code is not a mere convention. It is as essential to the advance of natural knowledge as the Experimental Method itself. The latter can produce isolated pieces of knowledge, the former provides automatic mechanism for co-ordinating such fragments. Also, it provides the stimulus which drives men to continue to do research, to write books on the results, to organise such implements of communal knowledge as journals, conferences, indexes, and museums, as well as their own laboratories.

So long as research of the unrestricted type (p. 315) is in progress, with all three degrees of freedom, the Code is fairly easy to obey, for those who have grown up with it. Any restric-

[1] Some aspects of this section's contents were published as "The Nature, Scope and Difficulties of Industrial Research," *Int. Conf. Fed. Master Cotton Spinners' Rep.*, Zurich, 1921.

tion in one degree or more—in method, aim, or subject—at once introduces difficulties. When the Aim is restricted to results which are commercially usable, then a policy of secrecy often displaces the free diffusion of knowledge, in order that the country, industry, or firm concerned may monopolise the results of the efforts made for it. The Code can no longer operate freely. The extent to which its operation is hindered, thereby making industrial research artificial and formal, instead of being a natural and spontaneous activity, varies from one industry to another. The hindrance is least in agriculture and in those modern industries which have developed from the research laboratory so directly as to have kept contact with its methods and ideas. It is greatest in those old industries which had grown to maturity before the modern renaissance of science. That agriculture should be an exception, though it is among the oldest, must be ascribed to the farmer's direct contact with natural forces.

Within many of the old industries it appears incredible that a free interchange of knowledge with competitors should ever be good business for the individual, and it will take many years, combined with the pressure of changing conditions, to make it credible. Yet I have good reason to believe from my own experience (p. 346) that such a change is under way with the cotton industry of England, though lagging far behind the changes elsewhere which will presently be noted. Many minor considerations of practical importance are in favour of the altruistic, communistic policy in dealing with the results of industrial research, as contrasted with the present tendency to try such a policy for getting the results, but the chief consideration lies in the research itself.

It has just been pointed out that secrecy is utterly incompatible with the unrestricted research which is typified by the university laboratory, the M.A.S. class. But in the act of isolating other classes of research (Section I) it has become self-evident that the application of research to industry does not deprive research of its essential features, but merely restricts the less important of them. Therefore, if industrial research were to attempt to carry on under secret conditions, it could only do so by becoming a parasite upon pure science. In actual practice the conduct of secret "research" is a logical impossibility, or, rather, a futility, since it would mean the extinction of every

technical or scientific journal and learned society; secrecy thus becomes a compromise, and a troublesome compromise. For parasitism has obvious limits which, if exceeded, will destroy the host. It may be considered that to draw an analogy with parasitism is unfair, and that the relation of industry to science is rather one of symbiosis than parasitism, in the sense that the former purchases the work of the latter; this alteration of the analogy makes no difference to the argument, for no purchaser prefers a badly-tempered, soft, blunt tool when a keen one is available; even research which is conducted under conditions of merely partial secrecy is seriously blunted. It is not inconceivable that the scientific worker of the distant future might eventually formulate a code of professional conduct, like the medical profession, and rank it as "unprofessional conduct" to work under secret conditions, disqualifying the worker who did so from membership of learned societies, and the like.

My reading of this problem, as being fundamental to the social utilisation of research, may seem to be fanciful, yet it is merely a restatement on the one hand, while, on the other hand, much which was mere fancy when I first formulated my conclusions about it has since become commercial practice across the Atlantic. Thus in 1908 Mr. H. G. Wells wrote that "The main difference of modern scientific research from that of the Middle Ages lies in its collective character, in the fact that every fruitful experiment is published, every new discovery of relationships explained. . . . Scientific research is a triumph over natural instinct, over that mean instinct which makes a man keep knowledge to himself and use it slyly to his own advantage. . . . To science it is a crime." [1] And it will not harm Mr. Wells' argument to reinforce it by the practice of the wealthiest man in the present world, whose plate glass is made for the Ford car in a works which runs its own improved process with every detail open to inspection by any casual visitor, so that "if anyone can improve upon the process and manufacture the glass for himself at a reduced price, the Company would scrap their own plant and purchase from the new producer, thereby effecting a saving." [2]

The fundamental causation of an industrial progress in America, which has been described by the editor of *The Economist*

[1] Wells, H. G., *New Worlds for Old*, London, 1908, Chap. II.
[2] Austin, B., and Lloyd, W. F., *The Secret of High Wages*, London, 1925.

as "an economic revolution," [1] has been enunciated under the headings of nine business principles for industrial management. The sixth of these is that "A free exchange of ideas between competing firms should be advocated"; the last is that "Research and experimental work are of prime importance to progress."

A specific application of this profitable altruism to cotton-spinning in America is provided from a recent report by two visitors [2] thereto. "The interchange of ideas is very free. . . . Their attitude is this: You get just as much out of the other man if he goes round your mill as he gets out of you." "As soon as a man has a new idea he goes and tells his neighbours about it at a convention, while he himself receives perhaps a dozen new ideas from other speakers." [3] This is simply the childish attitude which has made the alchemist into a chemist, and the medicine man into a medical man.

It would seem already to be a proven fact that business prosperity can at least exist under the handicap of scientific publicity, whereas research does not prosper under the handicap of secrecy. Nevertheless it may be of some service to the older industries to continue knocking at a partially opened door, by setting out some of the less recondite reasons which make such altruism [4] seem less unreasonable.

The long-sighted industrialist knows that a continued monopoly of knowledge is impossible, and is content so long as he "can give the other fellow his dust." Such monopoly becomes increasingly difficult with the entrance of more research workers into industry; such men are skilled detectives, to whom the activities of a rival firm present a far simpler puzzle than those which Nature sets them daily.

The individual research worker is handicapped by a secrecy policy, since he cannot discuss his work with his fellows, and this in its turn is a handicap to the industry, which has difficulty in obtaining and keeping active researchers if they cannot justify their existence to their fellows outside. He is also handicapped by inability to realise the full implications of his own works, and the gaps therein, because, as every such worker knows, the act of "writing up" for open publication is totally different from

[1] Austin and Lloyd, *loc. cit.*
[2] Prof. W. E. Morton and Mr. H. G. Greg.
[3] *Jour. Text. Inst.*, October 1926, pp. 150 and 154.
[4] W. L. B., "On the Profitable Nature of Altruism," *Jour. Trop. Agric.*, 1926.

preparing a report for a limited audience, of known knowledge and bias.

The overlap of effort between parallel firms is very wasteful, because it is not the useful overlap which leads to criticism and safer advance, but the overlap in which various groups study the same things from very slightly different angles, and with very slightly different aims, so that the usable fraction of each one's work is slightly different. Years are thus taken to reach results which might be achieved in months by common discussion and criticism. The expression " usable fraction " is important; everyone with experience of industrial research knows that only a small portion of the total work done can ever be used, owing to limitations imposed by such things as the cost of production. The worker's knowledge that all the " unusable fraction " will have to be done again somewhere else, by someone else—and probably quite soon, since convergence of ideas is so common—produces a feeling of wasted effort, intensified when the credit for his original discovery eventually goes elsewhere.

In spite of the little which any one worker can achieve, yet in some cases the possibilities realisable by research are already ahead of the opportunities for their materialisation by isolated effort. When the history of industrial research comes to be written there will be many examples of discoveries which have been made repeatedly until discoverer and interpreter happened to coincide with an industry free to realise its assets.

Section III

THE CONDUCT OF RESEARCH BY TEAMWORK [1]

THE enunciation of the Researcher's Code in the previous section has a very direct bearing upon the proper use of a "team" of research workers. The subordination to leadership which is an essential characteristic of a team must somehow be reconciled with the individuality of effort which is one of the three components of the Code. Communism of results within a team presents no serious difficulty, though the allocation of individual credit is somewhat more awkward (p. xii).

The previous classification of research also helps to make it clear that teamwork is almost inevitable when industrial subjects are to be studied. It is not perhaps entirely necessary, for it was a clergyman of Kent who produced one of the primary inventions of the cotton industry, and it is very likely that the very big industrial discoveries will continue to originate from my M.A.S. class of studies outside any industry, where a better perspective is available. But it is impossible for exhaustive studies within the industry to be made by individuals, because industrial research demands such a variety of knowledge as no person can possess without becoming a mere dilettante, and becoming obsolete in his special branch of learning while so doing. A simple piece of such research which was dismissed by a line in the text of this book may be cited for illustration of this variety:

A defect of existing practice had been noticed and its improvement suggested to me. A lead was first given to the team on the basis of general technical impressions and some elementary physics. Two palliative inventions were quickly suggested by the mill and by a laboratory. The drawing-office then detected a clue to the reconstruction of the defective part of the machine involved, and a complete re-design was worked out. Statistical

[1] Based on a paper to the Association of Economic Biologists in 1922.

observations of the occurrence of the defect were meanwhile made in order to evaluate it, and were interpreted by the collaboration of three persons, and of the mill staff. A minor possibility was eliminated by physical experiment, and applied mathematics provided exact data for use in the drawing-office. Following upon actual workshop construction, technological knowledge was required in trials of the new device, and an accessory invention by an operative improved it. Subsequent trials in real mills increased the size of the team concerned to some fifteen people: statistician and mathematician; physicist, photographer, and skilled observer; engineer and mechanics; technologist and operative; mill manager and overlookers. The essential part of the research was thus completed in a few weeks.

The variety of aptitudes required by industrial research is not confined to that given by formal training; indeed, the division of a team by subjects, such as chemistry or electrical engineering, is rather artificial; the tools they use would give a more natural grouping—bottles, wires, lenses, cutters, etc. This variety includes manual as well as mental ability, imagination as well as experience. The types of mind which it can usefully employ have scarcely an attribute in common except detestation of untruth and a susceptibility to slight hints. Some are slow to notice a new line of inquiry, but will follow it grimly when put on the trail; others find the most trivial observation suggestive. Some enjoy the assimilation of previous work to be used as a foundation for their own; others, for preference, look forward. Some are experimentalists born, who appreciate the mere acts of manipulation; others care only for the results thereof. Some have the patience for repeated observation without staleness or bias; repetition drives others to invention. Even the unimaginative can be used.

The co-operation of such variety within a research team can take various forms, and the organisation of three such forms has fallen within my experience. There can be a voluntary loose relationship, as among those members of a local scientific society who were interested in a particular problem; this is the highest grade. At the other extreme is found such a corps of uneducated assistants as Mr. Holton helped me to make out of picked fellaheen.[1] In between these limits comes such an organisation as

[1] W. L. B., *Egypt of the Egyptians*, London, 1915, p. 221.

the department (p. 338) in which much of the work described in this book was done.

In the first of these cases the Researcher's Code worked automatically, as between equals. In the second, its principles still held good as the elements of handling native subordinates; to discourage the slack, promote the keen, and make an *esprit de corps*. In the intermediate condition of the formal organisation, both extremes were present, the staff ranging from specialists older than their chief down to juvenile assistants. Here the Code was continually applied in order to escape from the rigid formality of work done under orders, in a prescribed way. Such work is a necessity of military organisation, and largely unavoidable in business, but it reduces any team of research workers to a condition of mere technical mechanism.

The ostensible anomaly of doing individualistic research under team conditions is no real anomaly; this is evident when it is realised that the whole body of research workers in all parts of the world is itself a team by voluntary association; even controversy within that team has, for its ultimate result, the advantage of the team as a whole. Just as the Researcher's Code has given rise to an organisation which is almost the only self-acting one achieved by mankind, an organisation wherein no chiefs, subventions, or social sanctions are required to keep it alive and active, so a team conducting industrial research under the same Code also becomes a self-acting organism, and not a formal organisation. Some such maxims of research organisation as the following have been found serviceable in giving free exercise to the craft of research, and yet directing it to industrial aims.

(*a*) It may perhaps be taken as axiomatic that a policy of decentralisation is essential in a research team, each member controlling and presenting his or her own work. Team-leading thus becomes an exercise in grasping the work of individuals, to stimulate effort or to assign credit, and to facilitate common knowledge of the results by interpretation and generalisation.

(*b*) The specialist in applied science must of necessity become a "Jack of all trades," widening his range of superficial knowledge as his specialisation increases. This paradox is inevitable, but it necessarily limits the achievements of the industrial specialist, as a specialist, to lower levels on the average than those of

pure science, where exclusive specialisation is possible. It is inevitable because industrial research results cannot be handed on to fellow-workers within the team unless they have been translated from their acutely special form.

(*c*) Arising out of this need for a general appreciation of many interests outside the special subject, so that team-work among a limited number of persons may be facilitated, there may be mentioned a rule which is good, though not always practicable: work should never be given out to a subordinate which the senior cannot do for himself. In the simple case of manipulative work this rule includes the making of a "time and motion" study. In the higher stages, where the team is a free collaboration, such omniscience is impossible, but the rule makes for understanding and efficiency in the lower stages.

(*d*) No research team contains any person who does not bring in some essential contribution. That contribution may be in manual craftsmanship, in orderly work among the files, stores, accounts, or library books, or in such habits of thought—critical, analytical, or constructive. A side-issue of this is the important inference that a research team can have no sharp boundaries, such as a distinction between scientists and operatives; the work shades off too imperceptibly (Section I), and the attack upon problems is made by enveloping movements.

(*e*) The leadership of a research team consists in administering research, not in administration. Details of accounts, stores, and maintenance generally should be the charge of an administrative assistant who is given full responsibility except in broad policy and budget totals. The tools of research for the leader are his subordinates' brains, and the choice, control, and study of those tools is his function. To saddle a steady-goer with work requiring inspirations of absurdity, or to demand accumulations of data from an erratic genius, is almost as wasteful of material as it is to retain the services of a secretive worker. Each member of the team must do his own work, and be helped by the leader to see round corners to the work of some other member. The leader does his share of the research by piecing together the jig-saw puzzle of everybody's results, thence absorbing and suggesting new ideas. Some manual laboratory work is advisable for the sake of keeping him in touch with limitations. Always he has to balance the demands of industrial efficiency against the danger

that he may prevent the individual worker from thinking for himself or herself.

(*f*) The researching efficiency of individuals, once chosen, can only be slightly controlled. Education necessarily happens in course of time, but the main control of efficiency is obtained along the lines of so-called "scientific management"; it is a crime against time for the biologist always to cut all his own sections, or for the engineer to do all his own rough turning. Junior assistants can do far more than they are commonly given to do, and they learn in the act of helping. Accessories, apparatus, automatic operation are the main controls available; team-leading in research is an unending study of method. But the word "all" should be noted; it is as easy to slide down the other slope through over-specialisation and departmentalisation, whereby one misses those side-issues which are usually more important than the original objective.

(*g*) Every new problem presented to the team needs a new method. Perhaps the greatest advantage of teamwork lies in the availability of mutual help for this purpose. In this connection the word "method" does not mean apparatus only, or the graphic and statistical treatment of results; its implications cover the planning of interlacing or converging experiments, and the transmission of problems and results from mind to mind. The actual apparatus for industrial research may often be simplified and cheapened for its single-purpose use, and yet gain in effectiveness and in its time-saving qualities. It may be worth while to spend months in looking for a method, to save time in the long run; I have seen six months come back in a week, and six weeks in a couple of days.

(*h*) Perhaps the most important maxim of this pragmatism is: that a research team, even less than any other kind of team whatever, cannot function efficiently under compulsion. It must be animated by good-will.

Section IV

THE UTILISATION OF RESULTS

It were illogical to present these notes without mentioning the process of utilising the results obtained by research, but its difficulties are so conditioned by the degree of secrecy under which the results are obtained as to make a short section sufficient.

A long delay in utilisation seems characteristic of industrial progress under the ordinary procedure of invention and application. Simple examples can be drawn from the automobile, where such devices as air-intake filters or four-wheel braking, now become standard equipment, have been rejected repeatedly as impracticable or unnecessary. It would seem that this time-lag is primarily due to insufficiently wide diffusion of the results obtained, after they have been developed to the chrysalis stage. A result may have possibilities which are evident in that stage, but its conversion to an accomplished fact involves further research on materials, and methods, and costs, both in construction and in use, such as are not always within the interest or scope of the primitive researcher. Moreover, these later stages are usually carried out under conditions of privacy which debar the community from profiting by such important experiments. A social waste of effort is thus involved by the hope of obtaining a monetary reward, which waste would disappear if the development studies were common property. It is usually held that the stimulus for development would vanish in the absence of a chance of a prize, but it is at least debatable whether the game itself is not a more powerful attraction to the players themselves than the cup for which it is played; the movement towards the abolition of prizes in school-work may serve as another suggestive analogy.

There can be little doubt, apart from this, that if the Researcher's Code were applied throughout industrial development, such development would be enormously accelerated. Thus, the

designing of experimental works-equipment often involves problems which are practically insoluble except by open discussion with some trade or interest quite unfamiliar, even to the members of a versatile team. Each new material of construction is itself an addition to such complication; such materials as duralumin or the chromium steels, with the cellulose, silica, and rubber products, may be instanced.

It is sufficient to note the existence and implications of this broad fact, that the time-lag in the application of knowledge is often unnecessarily long. The more immediate interest of the research worker is in the fundamental utilisation which is effected automatically by diffusion of knowledge, whether inside the team itself or externally to interested persons.

Internal diffusion of results is done best when done informally by talking "shop," with the help of the team-leader. Formal "staff meetings" serve rather to obtain a written account for permanent record, of work which has advanced to a "publishable" stage. For the preservation of minor results and for ephemeral record, I have found it serviceable to employ a system of individual's reports based on diaries, and made up fortnightly into a routine duplicated Report, as the equivalent of a "Journal." These two types of reports serve also for preservation of results as well as for diffusion within the team.

While it must be taken as axiomatic that free internal diffusion is a necessity if the coherence of any research team is to be effected and preserved, the external diffusion of results to "constituents" of industrial research is equally necessary, but entails a risk of leakage. Such leakage is, of course, merely the desirable diffusion of knowledge seen from another point of view. Yet, the more important any new knowledge may be, the more is it desirable that it should not be sent out as an isolated assertion. The difficulty is, of course, quite insoluble even by compromise, until the results of industrial research are as freely diffused as those of pure science. This Utopian state of affairs appears to be arriving in the U.S.A. (p. 321).

An extension of the "team" conception to workers other than those professionally engaged in research may be expected eventually to solve the inherent difficulty that the interpretation and translation of research results, their subsequent development to the industrial stage, and especially their commercial exploita-

tion, demand a different outlook and interest from that which the most typical research worker usually enjoys. This applies even more acutely when the research team is so organised, and so well equipped, that it can carry its results forward to the small-scale works process before handing over; the technical interest is then fully in sight of submergence under less fascinating limitations.

Section V

THE COST OF INDUSTRIAL RESEARCH

It is a common belief of industries and firms who are advised to embark upon research work that the expenditure is prohibitive. It may therefore be useful to indicate, very generally, where and how the conduct of research is costly or otherwise. While the allocation of costs must depend to some extent upon the subject studied, yet it has seemed to me that research work upon a laboratory footing is too often conducted extravagantly, through the use of insufficiently flexible equipment. My experiences in forming this conclusion are based upon the formation of more than one type of laboratory, all of them beginning from bare walls, or even from bare ground; these included a botanical laboratory in Cairo and a general laboratory in Manchester; a twenty-five-acre experiment station at Giza, for genetics, physiology, and seed-supply; lastly, the adaptation at Bollington of an old house to a colony of laboratories, workshops, and spinning mill.

From these adventures in organisation I conclude that so long as adequate salaries and wages can be afforded for the staff, then the cost of equipping and conducting research upon a laboratory footing is negligible. One year's salary list is more than ample allowance for the capital cost of equipping a laboratory in an existing building, and its running expenses are only a small fraction of the salary charges. For this assessment to hold good it is to be understood that at least one member of the staff has an interest in the design and construction of apparatus, and can use the simpler tools of a workshop (p. 365), up to a small lathe; moreover, this interest must march with a certain ability to extemporise, rather than with a conscientious craftsmanship, such as may perhaps be exemplified by the abundant use which it is possible to make of Meccano parts in constructing apparatus.

The cost of constructing buildings is not included in this assessment, but here again—if no existing building is available—

it is possible to use a simple building of factory type. Such buildings need not be unpleasing, either externally or internally, and they are not only possible but advantageous, on account of the ease with which they can be extended or altered to meet the changing requirements which are such a characteristic feature of industrial research. The same principle of elasticity applies to furniture, and the sectional system of interchangeable units which I have described elsewhere [1] has now been tried out under varied circumstances, both at home and abroad, and has shown advantages of economy and flexibility which far outweigh its minor disadvantages.

The library is a very costly item if it is undertaken with the purpose of making a real reference library, such as is necessary in isolated situations over-seas. When other libraries and booksellers exist within a few miles' distance, as in "home" conditions, a great economy of capital can be effected with very little loss of efficiency, if books are freely ordered as and when they are required. The cost, except for a small nucleus of encyclopædic books, is thus transferred to current account, and even so is quite small in relation to salaries; the wages of two office-boys will keep an unrestricted library available for a team which includes several active researchers.

The fundamental expense in laboratory work is thus upon salaries, and other costs can be comparatively negligible. It is, of course, an easy task to spend a great deal upon laboratories, gaining much in appearance though little in efficiency, but the real test of equipment lies in the ability of the laboratory to undertake new lines of work at short notice. For this test to be passed it is necessary to have such a flexible equipment as I have sketched, in conjunction with a few expensive items which cannot be extemporised; such as lenses and prisms, resistance coils and galvanometers, balances and micrometers. Such implements as the "thread galvanometer" and the Zeiss "2 mm. apochromat" objective have been in all my laboratories.

The low capital cost of laboratories is in very marked contrast to the high capital cost of any equipment installed for the purpose of applying theory to practice. Since industrial research is usually started with the intention of making this application, the

[1] "Equipment of Research Laboratories," *Rep. 2nd Conf. Indust. Res. Assocs., Dep. Sci. and Ind. Res.*, London, 1919.

low cost of plain laboratories is in one sense irrelevant. At the same time it is a distinction which should be stressed, that the cost of industrial research lies not in the search for knowledge, but in the utilisation of knowledge when found. I have found that when a good balance between "theory" and "practice" was attained in an organisation for both, the workshops alone required more than twice the capital which had sufficed for all the laboratories, while the works equipment cost more than twice as much as the workshops. Laboratories, workshops, and mill thus stood, in the ratio of their capital costs, somewhere between 1 : 2 : 4 and 1 : 3 : 9. Of the whole capital expenditure about half was on the property account, a quarter on the equipment for application purposes, and the rest upon maintenance and welfare, excepting a mere 3% upon the equipment provided for all laboratories during ten years' working.

The balance of expenditure between knowledge and application is quite different in the current account. The salaries and wages paid in laboratories are about the same as those paid to twice the number of persons on the applied side; this is because the latter has a higher proportion of operative workers who need not do more than carry out instructions. The allocation of salaries and wages thus divides into three equal parts for laboratories, for application, and for maintenance, welfare, and direction. This accounts for three-quarters of the current expenditure, and the remaining quarter goes principally to materials, whereof the laboratories use an insignificant fraction apart from their share of light, fuel, power, and rent.

Thus it would seem that so far as industrial research is of a fundamental kind, falling within my M.A.s. class, or not going very far beyond it, so its expenditure need be little more than that which is required to house and pay the research workers. This is again affected by the working conditions, for if the work were done without secrecy, there would be no need to attract the best type of workers by paying more than for pure research. In so far as industrial research passes over to the more restricted classes of work, so the workshop costs increase, and then those of the works themselves. Application is costly, knowledge is not. But since "knowledge is power," whereas application should lead directly to profit, this distribution of the cost of research is entirely equitable.

Section VI

THE EDUCATIONAL STIMULUS OF RESEARCH

THERE is one very important effect whereby the establishment of a research organisation may make itself felt throughout an industry. This effect is the stimulation produced, almost unconsciously, by the knowledge that there exists a court of reference, or a scrap-heap—for either metaphor may be correct—to which ideas can be referred for consideration and advice. I have been assured by a competent authority that this general stimulation of interest in the industry is by itself sufficient to pay for the upkeep of a research side, even if no profitable original discovery were ever made therein. The main text of this book shows that it is still possible to make actual discoveries of facts which are not only new, but are of practical interest, concerning industrial processes which have long been in everyday use. Also, that it may sometimes be found possible to rearrange ideas concerning the relative importance of properties possessed by the raw material. Such discoveries or heresies eventually challenge technical interest, and lead to the development of a researching attitude outside the walls of the research department; this applies more notably and effectively to the younger men.

Again, since the active conduct of an industry is necessarily occupied with intimate detail, its human factor is in the opposite position from that involved in the pursuit of a hobby, where the zest of the golfer or " radio fan " is in that very concentration on detail which is the monotony of bread-winning. But the application of research to industry demands active help and interest from the human factor, whether management or operative, and in order to enlist this help effectively it is necessary to get away from detail, generalising the results of research as broadly as possible. But this in its turn demands that there shall first be constructed a " science " of the industry, from which such generalised presentments can be taken, and whereby those

ostensible exceptions which the practical man often adduces so disconcertingly can be dealt with promptly.

Thus the growth of any research organisation to the stage at which it can exert an educational influence—not a schoolmasterly one—must be very slow. In that growth it passes through successive stages wherein it surveys existing materials and processes, analyses the reasons for conventional practices, tests these reasons under the existing conditions—which may have altered since the time when the convention was correctly established—and investigates roughly the possibility of new materials and processes. By the time this stage has been reached, and not any sooner, it is possible for the research organisation to come into direct contact with the industrialist without too much forfeiture of appreciation and respect on both sides.

But to generalise the results of study with the object of making them available to others is the problem of Education, the " leading-up " of the mind, a process which proceeds automatically when once well started on such interests as books or etchings. In so far as industrial research can build up these generalisations, in contradistinction to the details which are its raw material, so it can make its industry self-educating by virtue of interest in its own problems. Such generalisations serve the purpose defined in George Meredith's phrase of " chalk eggs, which lure the thinker to sit." This is almost the opposite to " vocational training " for an industry, which tends to lead down to detail.

The work of industrial research thus fuses eventually with that of industrial education, in that a rational inter-relation of details through research work enables the student to forget about them until they are wanted for use. But the task of constructing a teaching system is no light one, and it is even harder when an old subject has to be rearranged, or taught differently, more easily, and more fully. Such subjects as Euclid's geometry and Textile technology present analogous educational problems. The Textile Institute has done service in recognising the existence of a technical side to the textile industries, but to disentangle the subject from the mass of details and from commerce, to describe, practise, and teach it, seem almost insuperably difficult.

Indeed, such teaching can hardly be done in the industries by formal deliberation and set intention. The only practicable

method is by free intercommunication of generalised knowledge (Section II), not only inside the industry, among managers, operatives, and researchers, or between any of these, but also as between industry and industry, country and country. The new idea, or the broad generalisation, must be sown broadcast to find its own appropriate seed-bed, as in the field of pure science.

Section VII

THE GROWTH AND STRUCTURE OF A RESEARCH ORGANISATION

THE genesis of the Experimental Department of the Fine Cotton Spinners' Association was described in the Introduction. The following account deals with its growth-history and with the structure attained at the end of my ten years' service, much in the same way as for the cotton-hair in Chap. II.

The original work-room in Manchester, with one lab-boy, sufficed for the first two years. A computing clerk was then added, followed by extension into an adjoining room for a chemical laboratory with a chemist, another lab-boy, and a clerk, while the nucleus of a testing laboratory developed. In the fourth year came a few small machine tools with an engineer and another boy.

Increasing interest led to plans for combining an experimental mill, wherein trials could be made without disturbing production, with more workshop facilities and better rooms for the laboratories, together with surroundings more conducive to study and less subject to vibration than in the centre of Manchester. The formation of an Experimental mill had been advocated by Mr. Vernon Bellhouse long before my time, which greatly facilitated progress. I took as the basis of my planning an annual expenditure equal to one-tenth per cent. of the Association's capital, as being a reasonable insurance premium against ignorance, and less than this amount was actually expended in any one year. Estimates of capital expenditure were mere guesswork during the post-war boom, but even after five years' working the total capital had only exceeded my upper figure by a small amount, more than covered by economies on the annual expenditure.

The site was located at Bollington, the most southerly cotton-spinning village, especially appropriate as containing the mill which spins the finest counts in the world, as well as two other

mills belonging to the Association. Except for its perfect cotton-spinning climate, the locality had great disadvantages, being some miles away from collieries, with open country to the west and high moors to the east.

Buildings

The existing house of "Rock Bank," a stone structure about a century old, had been built on the slope of the escarpment of the Derbyshire hill-country, overlooking the plain. The back garden was on the same level as the first floor of the house, thus making a convenient site for additional buildings. The foundations rested on solid rock, so that the cellars were as free from vibration as could be desired. Outbuildings included a two-storey stables, a building containing gas-engine, dynamo, and cells, which were reconditioned, and a garage; there was also a greenhouse.

Any attempt to install mill machinery in the house itself would have been a compromise, so the Association decided to erect a separate and specialised building for the experimental mill. This was placed behind the main house on the first-floor level, without impairing the appearance of the property. It had a saw-tooth roof, cavity walls, double glazing, and air-lock doors to its four rooms, with facilities for automatic control of temperature and humidity.

About a mile from "Rock Bank" was an available building which had formerly been an elementary school. This was floored and cut up into a number of separate rooms to serve as a Hostel. A hard tennis court was afterwards laid down in its garden, out of house-keeping profits.

Power, etc.

Power was available from a near-by mill, up to 80 kilowatts, thus giving a comfortable margin over the 50 kw. needed for the machinery installed, in the event of future extensions. As this power was only available during mill hours, the existing electric lighting cells were normally kept charged by driving the dynamo from a motor off the power supply. The gas engine was available in emergency, so that a lathe or frame could still be run, as well as light and laboratory motors.

The existing central heating of the house was much enlarged

and connected to the new mill building, so that all was controlled from one pair of furnaces, while the labour of stoking was reduced to a minimum by excavating a coke store in the courtyard adjacent to them.

Water and gas were available from public supply, with a pressure regulator on the latter. Cleaning was in charge of the caretakers.

Housing

The ground floor of the main house was almost entirely allocated to the workshops, the floors being reinforced where necessary, while two cellar rooms in addition served as carpenter's shop and forge. The testing laboratory and general office completed the use of this floor.

On the first floor were located the rest of the laboratories, general, physical, and chemical; the library; the offices of the chief and secretary, and of the spinning section. The mill building itself was on this level and directly accessible from this floor.

The top floor contained the caretaker's flat, a small lecture-room, and the files of samples.

The cellar rooms, other than those used as workshops, were used as physical laboratories.

The top floor of the stables became a residential flat for the administrative assistant. Its ground floor served as a cycle-room, garage, and stores.

The garage was converted into an annexe to the chemical laboratory.

The " Old School House " became the hostel.

Staff

The general plan comprised three main Sections—Laboratories, Mechanical, and Spinning—flanked by minor Sections for Maintenance and Welfare. Each Section had its own head except the Laboratories; because it was more important that the foundations of the Department's knowledge should be well and quickly laid than that it should do propaganda without adequate knowledge, I acted as head of the Laboratory Section as well as of the Department. The Mechanical and Maintenance Sections were also doubled.

The growth of the Department had been so natural that every person added to it simply took up work already being

done by someone else, whether as scientists, craftsmen, or orderly folk, in order to do it better and more thoroughly. I had myself sloughed off in succession the occupations of typewriting, computation, instrument making, mill observations, testing, physical experiments, and microscopy, among others, handing them over to more competent additional staff, or to junior members who had grown up into competence. That the very composite nature of the staff, living in close proximity in a small village, as well as working together, did not give rise to insoluble personal difficulties, was simply due to their loyal acceptance of the team attitude which had resulted from this method of growth.

Pay.—A fairly consistent scheme of salaries and wages inside this varied team was reached by taking the pre-war rates of the National Physical Laboratory, plus 30%. This was too low when first adopted, but did not have to be revised when the cost of living fell, and so effectively speeded up the promotion of those who had been in the Department a long while. Care was taken in its application to ensure yearly promotion within the grade for satisfactory work, and the passage into a higher grade for work of exceptional quality or quantity. With junior staff who were studying for examinations, each approved success qualified for increased pay in the same way as a year of service.

Education

It was evidently impracticable to use unskilled adults in England as I had formerly used fellaheen in Egypt (p. 267), but the London University external degree system made it possible to employ a few picked boys and girls, and yet to avoid the risk of a dead-end for them. Whether they trained up into active researchers, or as craftsmen, or were eventually discarded, their use and their independence clearly hinged on further education. The London degree proved invaluable, by providing an objective and stimulus, while mitigating the disadvantage of experience acquired solely inside the Department. Assistance in study, and laboratory courses, were voluntarily provided by the seniors.

A second aspect of education was developed by the Association sending a few selected young men from the management of constituent mills for a succession of short courses, each course lasting a fortnight at intervals of a few months. Each Laboratory and Section was thus visited in turn, to familiarise the visitor

with limitations and ignorances, as well as with knowledge. It need not be emphasised that the planning of such courses was difficult, and even dangerous, when methods as well as aims were new to the student, and only the subject was familiar. Nevertheless they were not unsuccessful, and their effect may be noteworthy in course of time.

A third special educational activity will develop by using the Department to train individuals for improving the resources of mill testing-rooms and offices when dealing with "complaints and defects," by means of special courses in manipulation and methods, detection of periodicities, graphics and statistics. A rapidly increasing branch of routine work will thus be devolved from the Department.

The function and equipment of the separate Sections may now be considered. I have already given in the Introduction (p. xiii) the names of those persons therein whose constructive co-operation has been utilised in this book.

The Laboratory Section

It is more than personal preference which assigns the premier position in such an organisation to the laboratories. They were the nucleus round which the rest had crystallised; they checked, tested, and advised the other Sections; they necessarily produced many constructive suggestions. But the circumstances made it easy to realise how limited was their practical utility without co-operation from the other Sections; machinery, apparatus, stores, files, and comfortable working conditions were all difficult to obtain until the others grew up and provided them.

General Laboratory.—This was the vestigial survival of the original work-room, retained as such in order to take up the early stages of any nondescript work, and primarily concerned with the devising of new methods of observation. As the hair structure studies developed further, its chief concern came to be microscopy, coupled with the care of the greenhouse in which cotton plants were grown for the purpose, and with the photographic work of the Department.

It thus occupied one room, with an adjacent dark-room. The greenhouse was made partly self-regulating as to its maximum temperature by a thermostatic relay working a ventilating fan. The staff was my personal assistant.

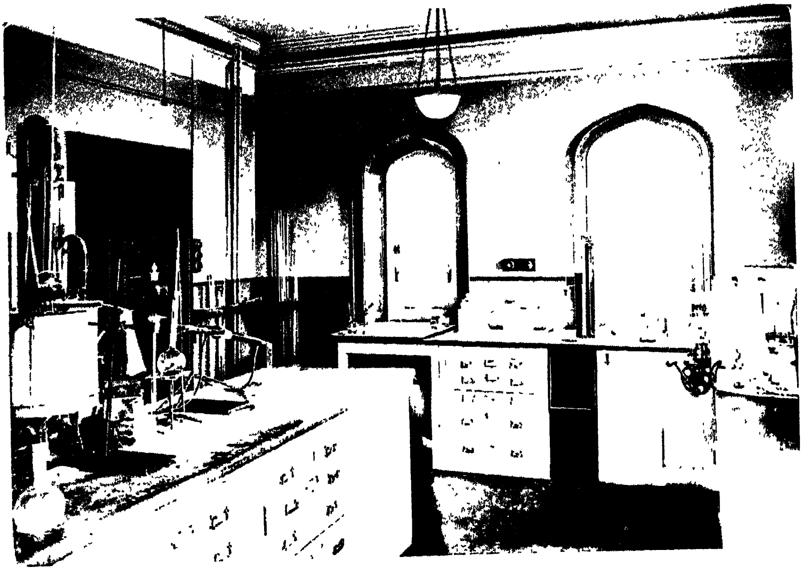

FIG. 120.—The use of sectional furniture in the Chemical Laboratory of the Fine Spinners' Experimental Department.

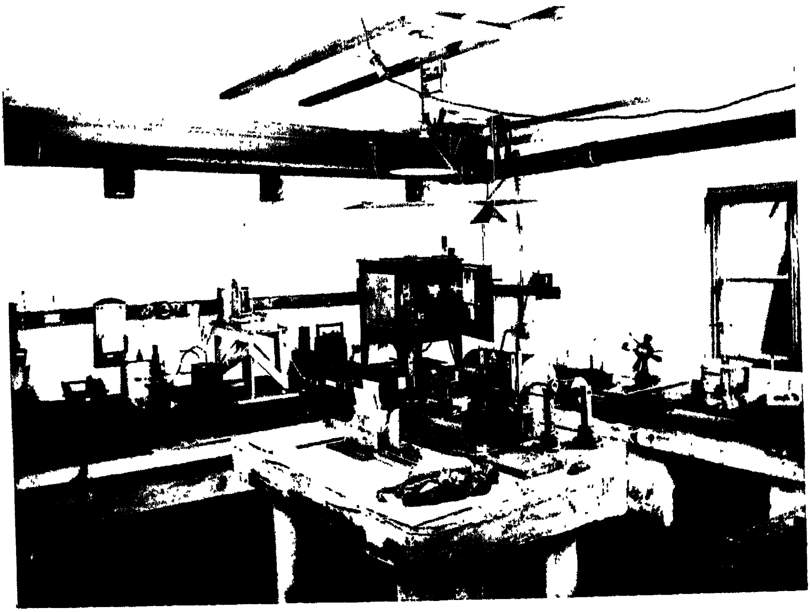

FIG. 121.—Cellar-room of the Physical Laboratory, formerly the house larder.

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Chemical Laboratory.—Built up by the chemist and her assistant, this was transferred to two rooms in "Rock Bank." It came to be largely concerned with defects in the finished product, aiding the Testing laboratory to track down responsibility. Researches on "finishing" problems and physico-chemical studies provided the foundations of such advice.

Compact use was made of the two rooms by opening out an existing chimney-breast between them to form a double fume-chamber, with radiators underneath. The demands of chemical equipment do not call for the same variety of furnishing as physical laboratories, so the unit sections were used as permanent furniture, including a lead-covered centre bench to which the gas, water, and gulley drain were led from the floor, leaving the room free for any subsequent modification. The annexe in the garage building at the other end of the grounds was provided with metered gas and water, gas boiler, electric motor and counter-shafting, and storage tanks, to house experiments on a small-works scale.

Testing Laboratory.—This was housed in the room which had the best natural light, to facilitate single-hair manipulation. The various pieces of apparatus were permanently ready for use on the benches or walls. In theory, its routine function was to test every sample of cotton which went into the spinning section, as well as the yarn which came out. In practice, the demands of other work prevented this from being done exhaustively, for specialised tests on "complaints" occupied an increasing part of its time, while it also served as the computation office of the Department. Its head had two girl assistants.

Physical Laboratory.—Housed in one room on the first floor, and two rock-founded rooms in the cellar, the equipment was chiefly notable for the way in which it carried on the Departmental tradition of extemporisation.

Its work had a deliberate bias towards the M.A.s. class of research, linked closely with similar work in the General laboratory. It also surveyed the physical conditions of the mill, as a guide to the practical interpretation of such researches. Being the last-formed portion of the Department, it was nearest to the original nucleus, for there can be no question that physics is the science most closely concerned with cotton-spinning problems.

The Mechanical Section

The development of this Section on its transfer to "Rock Bank" was so designed as to enable it to be run with a small staff consisting of the head and two mechanics only, by giving it ample resources in machine tools and stores. The range of the Department's interests was very wide, from the repair of mill machinery to the construction of physical apparatus. Most of the work was such as required craftsmanship, and the ability to work understandingly from rough sketches. When drawing-office work was unavoidable it had to be done by the head of the Section. Conversely, rapid and rough constructions were often needed, and a set of tools and a lathe were therefore assigned to the use of anyone from other Sections.

In the lathe-room were Hendey, Star, Benson, and Adams lathes; radial and pillar drills; planisher, buffing wheel, grinder, marking-off table, and fitter's bench. The heavy tool room carried a milling machine and bench miller, Norton grinder, power hack-saw, another bench, and the general utility lathe with its tools. A private shop had a Star lathe, bench drill, and grinder. A fourth room served as stores and packing-room. The drawing office and the heads' office were on the same floor. In the cellars were additional stores, carpenter's shop, forge, tool-hardening furnace, Roots' blower, screw press, and charging board for the many small accumulators used in the Department.

The building up and control of the stores were important, on account of the variety of requirements. The absence of some special steel might delay a laboratory experiment for weeks; the mill machines and laboratory apparatus between them employed every kind of screw-thread; a one-inch drill or a No. 70 were equally liable to be wanted.

The Spinning Section

Difficulties of building, equipment, and staff prevented this Section from operating before the seventh year of the Department's life. Its head had for staff a skilled spinner, two experienced girls for the frames and card-room, and part-time labourer's help. The technique of spinning tests had to be learned, and modified to suit special conditions and demands. The machinery had to be run in and defects remedied. A mass of procedure

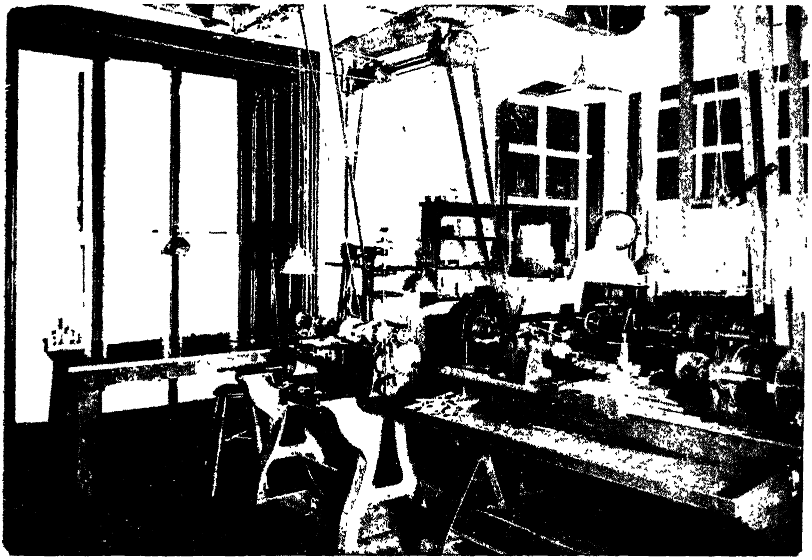

FIG. 122.—The lathe-room of the Mechanical Section.

FIG. 123.—Greenhouse with cotton plants.

[*To face p.* 344.

details had to be turned over before it was possible to run up and down the whole range of cotton spinning between 30s and 250s, from American to Super Sea Island. A system of " rationing " was arranged with the Empire Cotton Growing Corporation and with the Cotton Research Board of Egypt, fixing the maximum number of spinning tests to be undertaken voluntarily. The main routine thus gradually took shape, aiming at the most exhaustive spinning tests that were possible. Much progress was made in eliminating subjectivity from the test.

Other experimental work included the study of existing machines by running under modified conditions, or by collaboration with the laboratories in analysing their operation with the kinema, spark photographs, statistical observation, etc. New machines constructed in the Department, such as the sorter and rule spinner, were set up in a reserved space of one room, which also contained the locked cage of samples awaiting test.

Maintenance Section

This dealt with all general correspondence, orders, accounts, and wages, in charge of its head and one skilled assistant. Power, light, heat, and buildings were maintained through the caretaker and cleaners, while the gardens were also this Section's concern.

Hostel Section

Perhaps the most experimental feature of a generally experimental Department, the success of this section was due to its first Matron, Miss J. B. Cowan, and to the team spirit. It originated in the availability of a convertible building, and the housing shortage of 1920; the advantage of the change to Bollington would have been largely nullified if the junior staff had to travel long distances to work. As the staff was too small for two hostels to be started, I at last decided to try a mixed one. The Old School House provided six bed-sitting-rooms upstairs for women, and five downstairs for men, with kitchens, common room, dining-room, and Matron's office. The Matron's salary was provided by the Department, and on this footing the charges to residents enabled the house and garden to be improved continuously.

The advantage of the Hostel was found eventually to consist

in being a centre of common interests, round which a kind of collegiate life developed for the whole Department. Instead of collapsing after a few years' trial, it developed tennis, debating, and dramatic clubs, and became the social nucleus of the Department as a whole.

The Department as a Whole

Most of the considerations involved in the control of the Experimental Department as a whole have been dealt with in earlier sections of this Appendix. In it I was aided by the essential work of the administrative assistant at the head of the Maintenance Section, and by my secretary. The latter was also librarian, and conducted the "internal publication" of such papers as the Fortnightly and Staff-meeting reports (Section IV), together with a bi-monthly précis of the former, which gave a general idea of the scope of two months' activities on one sheet of foolscap.

My initial policy of deliberate isolation from premature contact with the constituent mills, as a whole, was held for seven years. By that time it was possible and desirable to communicate selected results of the research work to them regularly, leading in the direction of closer contact eventually. By the end of ten years the editions of internal publications in distinctively coloured covers amounted to twenty copies of the Fortnightly, sixty of Staff-meetings, and a hundred of the précis.

A round-table conference held every two months between the Executive Directors of the Association and the senior staff of their Department completed the routine part of this procedure. An important informal means for distributing information came automatically through adjudicating on "complaints"; I gave no encouragement to this use of the Department until the sixth year of its existence; then, when knowledge and staff were adequate, this function was allowed to grow, which it did by doubling the number of such inquiries every year for the next five years.

With the passage of time it had thus become generally evident that knowledge, to be of use, had to be made available for use. In this Appendix I have frequently emphasised my opinion concerning the hampering influence of secrecy upon research activities, and it is therefore the more necessary and pleasant

to put on record the fact that the absolute secrecy which was originally prescribed by the Association had been steadily relaxed year by year, especially in dealing with cotton-growers. At the end of ten years a degree of publicity had been reached, both to the constituent mills and to outsiders, which would have been incredible at the start, though it was still sufficiently restricted to be an onerous responsibility.

The decentralised organisation of the Department, and the team spirit in which nearly all its work was conducted, had made it competent to dispense with the leadership of any one individual in fulfilling its functions to the Association. With the broad outline of the yarn-cotton relation visible, as presented in the main text of this book, the personal objective of my late chief was secured (p. x). I was thus able to resign, with real regret at leaving the team, but with the knowledge that no foreigner to Lancashire could lead it effectively during the approaching phase of direct contact with the industry.

Section VIII

SPECIAL APPARATUS

Amongst the variety of appliances devised for the use of the Experimental Department (Section VII) were several which have been mentioned in the main text, as being the source of data which I have employed in discussing the yarn-cotton relationship. The following descriptions should be sufficient to enable the reader to follow the main features of their design and their limitations. In all cases where the invention of a colleague is concerned, the name of the inventor is stated. The items are arranged in alphabetical order.

Automatic Single-thread Tester.—The pendulum of this instrument, with its attached quadrant, swung in cup-and-vee bearings, and the load was applied to the yarn by moving these bearings away from the point of anchorage of the yarn; in these respects it was identical with the Magazine Hair-break tester (*q.v.*), though on a larger scale. The attachment of the pendulum to the yarn was effected by a pair of electro-magnets, mounted in rubber, and suspended from the laboratory roof; these took the place of the "hook" in the hair tester. The yarn was fed through a pair of rollers from the cop, on to a carrier mounted on an endless chain, which conveyed it between the jaws of the electro-magnetic clamp, whereupon the tripping of a switch caused the clamp to close, as also another clamp on the rollers. The beginning of the receding movement of the pendulum was synchronised with this clamping, and the deflection of the pendulum which indicated the load on the yarn was recorded by a spark on a disc, also as in the hair-tester.

The returning movement of the pendulum synchronised with the opening of the clamps, and with the closing of a clutch which again allowed the rollers to feed forward a fresh length of yarn. Series of observations were made at lengths of 60 cm. and at 360 cm. by altering the length of the endless chain.

The appliance was discarded after a sufficient number of tests had been accumulated, on account of the inconvenience involved in varying the length tested.

Barr and Anderson's Method.—In its original form this method was described [1] by its authors for measuring the internal diameter of glass tubing. The tube is immersed in a liquid of the same refractive index, the final exact adjustment to equality being obtained by the use of a monochromatic illuminator.

As modified for measuring the wall-thickness of cotton hairs it was only partially successful. With the collaboration of Mr. Hancock, certain mixtures of aniline and xylol were found suitable, used in a dry condition on dry hairs. A nicol prism in the microscope eye-piece confined the monochromatic light to the ordinary or extraordinary ray, as was more convenient, a less or more refractive liquid being used accordingly.

The practical invisibility of the central canal of the hair, when balance of refraction was attained, had to be overcome by polarising the incident light as well, to a sufficient extent to make the wall visible again.

Lack of homogeneity in the wall structure makes exact balance impossible. For example, the refractive index of the primary wall and cuticle is decidedly higher than that of the secondary wall.

Break, of hair.—See Magazine Hair-break Tester.

Clinometer, for Yarn.—The design of this instrument for measuring the twist in singles yarn was based on the impossibility of obtaining reliable data unless the length of yarn examined was only a few score calibres long. Otherwise the cylinder of hairs is not sufficiently sturdy, and may be given a negative twist at one end before all the original positive twist has gone from the other. Thus to work with lengths of only a few millimetres necessitated a design in which each examination could be made rapidly, and the examination shifted to another place along the yarn without delay, making a hundred observations per test as the normal procedure.

It was designated a " clinometer " rather than a twist-tester, because concurrent observation of the yarn diameter enabled the correlation diagram of twist and diameter to be plotted semi-

[1] " Two New Methods of Measuring the Internal Diameters of Transparent Tubes," *Jour. Sci. Instr.*, 1923–4.

automatically, and the range and mean value of the Twist Angle to be thus inferred directly.

The instrument is a special stage, attached to an ordinary microscope, and so arranged that manipulation is conveniently effected by both hands alternately, to save time. The microscope has a swinging nose-piece and two objectives, such as a 3″ and a $\frac{1}{4}$″, the first for observing the twist, the second for the diameter, in conjunction with an eye-piece micrometer scale.

On opposite edges of the field covered by the low-power objective are two clamps, one fixed, the other rotatable by gearing and a milled head. The axle of the rotatable clamp is tubular, to enable a continuous length of yarn to be drawn through both clamps. The length of 7 mm. held between them can be untwisted by rotating the milled head, and the whole field kept under observation until zero twist is attained; if any clamp effect is noticeable, this may necessitate twisting beyond zero and then returning to it.

The milled head can be arranged to spin back to the starting-point when released after the observation, by means of a spring or a weight, or this can be done by hand. In any case, a pointer is coupled to the rotating clamp by gearing which moves the pointer along a screw, thus indicating on a scale the number of revolutions of the rotating clamp at any moment. This pointer can be pressed into contact with the paper covering a drum, whose axis is parallel to the screw. If the drum has previously been turned by hand until a reading on its graduated head corresponds to the previously observed diameter of the yarn, the pin-pricks made by the pointer plot a correlation diagram.

After the twist, or the diameter, or both, have been noted down, or plotted by the pointer, the clamped length of yarn is released and a fresh place dragged into position. The fact that the twist has been restored to the length examined enables this to be done.

The release is effected by means of a slide, which removes a wedge whereby the fixed clamp was locked, and simultaneously drives the point of a needle between the spring-locked parallel cylinders which form the jaws of the rotating clamp. The yarn thus freed can be dragged through the clamps to any required distance further along, by winding it up on a small drum, on which a spring clicks at regular intervals. The slide is then

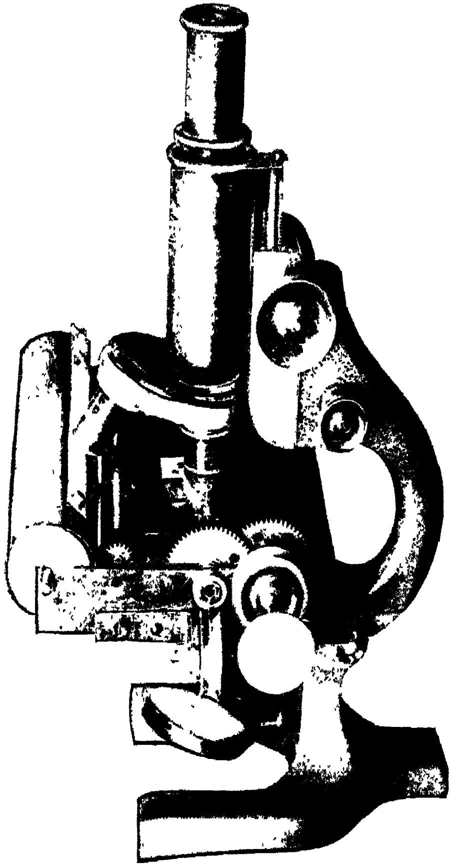

FIG. 124.—The Yarn Clinometer. A standard type of microscope with its stage replaced by the clinometer attachment.

The tubular axle of the rotating clamp is below the microscope objective, which is in the working position. The yarn brought through this axle is wound up on the click-controlled pulley immediately below it, seen above the microscope mirror. The jaws of the fixed clamp are visible between the rotating clamp's gear-wheel and the microscope objective.

The removable recording drum is to the left, with the index beside it, the latter being partly covered by the high-power objective which is swung out on the nose-piece; its screw and pinion are visible.

The large milled head for rotating the clamp and moving the index, with a pulley behind it for automatic return to zero position if required, is to the right of the microscope mirror.

[*To face p.* 350.

returned to its locking position, and the observations are repeated in the new spot.

Three or four double measurements can be made in each minute, the change from transmitted light for the diameter measurements, to black background for the twist, being made by moving a two-position hinged mirror when the objective is changed.

The zero twist can be determined with very fair accuracy, while the operation is sufficiently rapid to give good statistical material in a reasonable time. Sampling may be done at random, or the whole length tested can be weighed to provide a count correction, or comparative tests may be made on selected lengths of similar count. Reduction of the observations to " turns per inch " requires a little caution, since the unit length is only 7 mm., but eight years of regular use have shown no serious objections to its accuracy and convenience, even for such absolute measurement.

Colour-marking, of Yarn Thickness Variations.—See Periodicity Measurements.

Conductivity, Electrical, of Hair.—See Electrical.

Cone Winding.—See Periodicity Measurements.

Cross-section of Hair.—See Barr and Anderson's Method, and Porometer.

Diameter, of Yarn.—See Periodicity Measurements, and Clinometer.

Dynamometer, for Yarn, q.v.

Elastometer, for Yarn.—The intention of the design was to stretch a standard length of yarn at a fixed rate of strain, leaving the rate of loading dependent upon the strain of the yarn, as in the lea tester. Further, to apply the load in steps, releasing the yarn immediately the load at each step had been taken up. The length the yarn had attained under load gave the total extension, its length after release showed the permanent set, and the difference was the elasticity under the conditions of the test. In resetting for the following step it was impossible to extend the yarn accidentally above the length it had previously reached. One-metre lengths were standardised, with a straining-rate of 0·1% per second, and five such lengths gave a sufficiently smooth result, requiring about half an hour to test, compute, and plot.

The essential part of the apparatus is a balance beam, whose centre knife-edge support can be lifted steadily by a screw and nut, the former being driven at a fixed rate under the control of an electro-magnetic clutch. From one arm of this balance hangs the weight used at each step of the test. The other arm, with a similar bearing, holds the clamp in which is secured the upper end of the yarn.

The lower end of the yarn is fastened to another clamp mounted on a pivoted iron pointer, so balanced as to apply a load of only a few milligrams to the yarn when the latter has to support this pointer. The pointer can be held against a fixed stop by an electro-magnet—the holding-down magnet—or set free by opening the circuit. In this same circuit of the holding-down magnet is the clutch which holds the gear of the beam-lifting screw in operation until the circuit is broken. This circuit breakage can happen at two contact points; one is on the balance beam, and breaks when the pull of the yarn starts to tip the beam against the loading; the other is on the pointer-stop, and breaks when the holding-down magnet releases the pointer.

An index and scale are attached to show the rise of the balance beam, while a parallax mirror and scale behind the pointer define the position of the latter when off the pointer-stop.

A length of yarn being put in the clamps, and a one-gram weight on the beam, the pointer is lying on its contact, and the beam-contact is also closed. On completing the circuit by a hand switch, the beam-lifting clutch closes and the beam rises until the yarn carries a tension of 1 gram. The beam-contact then opens, thus stopping the beam from rising further by releasing the beam-clutch; simultaneously the holding-down magnet releases the pointer. This allows the beam-contact to close again, but without effect, because the pointer-contact remains open. The positions noted for beam and pointer under this nominal load of 1 gram are taken as zero readings.

The next step of the loading, usually 10 grams, is placed on the beam, and the test resumed by simply pressing down the pointer into contact, which when once made is maintained by the holding-down magnet thus energised. The yarn cannot be over-strained while so doing.

It should be noted that there is no inertia effect during the

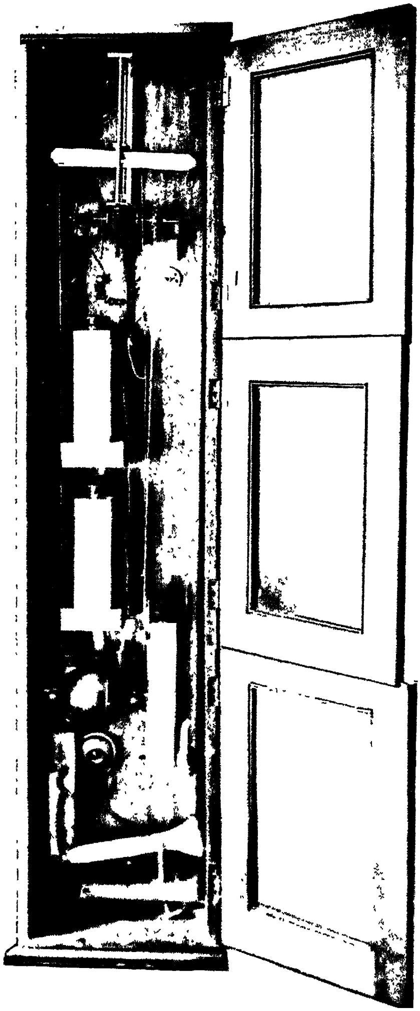

FIG. 125.—The Elastometer. Arranged in a wall cupboard.

The metre of yarn under examination extends from the left-hand end of the balance beam down to the elasticity pointer. Below the latter is its holding-down magnet.

Above the starting switch is the motor which, by a cord, drives the gear-box at the foot of the balance pillar. To the left of the gear-box is its controlling magnet. Above the motor are two cells for the electro-magnet circuit.

Near the bottom of the case on the right is the load, suspended by a fine wire from the right-hand end of the balance beam. The extension reading is taken on a scale to the right of the starting switch, over which moves a pointer rigidly linked at the centre bearing of the balance beam. The elasticity-set reading from the pointer is taken on a scale to the left of the same switch.

[*To face p.* 352.

straining, in which respect the appliance is unusual as a textile tester.

For comparison of yarn of different counts the readings must be corrected approximately by expressing the load inversely as the count, or by altering the steps of the loading.

Electrical Conductivity of the Hair.—The sensitive electroscope method devised by Mr. Slater for this work has been published.[1] An electroscope and cotton hair were insulated with amber, under fixed conditions of temperature and humidity, and the leak of the charged electroscope between fixed voltage limits was measured when it had been put to earth through the hair.

Hair Break.—A few points of method require notice, apart from the use of the Magazine Hair-break tester (*q.v.*).

The danger of unconscious selection of hairs, such as the stiff strong ones which are more easily picked up, has been mentioned in connection with Sampling in the main text. A tuft of the required number of hairs should be drawn from a prepared sliver, as if for the Hair-weight test (*q.v.*), and every one of these should be tested.

The clamping of the hairs for testing was at first done with sealing-wax, but this was found to involve a slight risk of local scorching, or at least of weakening. This was not entirely abolished when the wax-temperature was limited by pre-heating it in an oil bath. Any temperature above 100° C. was undesirable. Evaporating cements like seccotine or rubber solution were not satisfactory, on account of the long delay before complete certainty of dryness was reached. Eventually a low-melting-point wax which was yet sufficiently tenacious to resist the enormous stress concentrated on minute areas of hair surface was suggested by Mr. Slater in Everitt's Vacuum Wax, No. 2 Soft, used in the Cavendish Laboratory. When this is pre-heated in a water-bath, and applied with a pointed glass rod, it gives a rapid and safe grip on the hair.

Hair Conductivity.—See Electrical.

Hair Cross-section.—See Barr and Anderson's method and Porometer.

Hair Length.—See Sorter.

Hair Rigidity, q.v.

Hair Wall-thickness.—See Cross-section.

[1] Slater, F. P., *Proc. Roy. Soc.*, B, 1924.

Hair Weight.—The technique of this measurement has been simplified since I first employed it [1] to obtain a figure for the weight of 1 cm. of the average hair.

The counting of the hairs is done with the microscope and a moderately powerful objective, thereby avoiding eye-strain. The weighing is done on an Oertling Assay balance, which is sensitive to hundredths of a milligram by means of a 1-mg. rider on the beam. The calibration errors of a spring micro-balance are thus avoided, and as only one weighing is needed for each test, the loss of time is unimportant, compared with the fact that the results are in absolute measure, subject to correction for moisture content. The whole test can be completed and computed in less than a quarter of an hour.

The chief improvement is in the manipulation, and turns on the acquirement of some dexterity in so withdrawing a number of hairs from a prepared sliver that these hairs lie separated and ready for easy counting, instead of being tangled. With practice it is possible to withdraw a thousand hairs in four tufts, but the novice will find tufts of fifty sufficient. These hairs are laid down on a microscope slide placed immediately below the sliver, and are at once covered in their central part with a square cover-glass, care being taken that all the hairs project on both sides of the cover. After they have been counted, the projecting ends are pushed together to make a bundle of the hairs, the cover-glass is removed, and the bundle lightly twisted to harden it. A central region of 10 to 15 millimetres' length is then cut out with a pair of dissecting scissors, and the length of this trimmed bundle measured with dividers to estimated tenths of a millimetre.

The total length of hair in the bundle is then computed and logged, and other similar bundles made until about a thousand hairs have been included, totalling something more than ten metres of hair length. These are weighed and the hair weight per centimetre computed.

An alternative to cutting the bundle was devised by Mr. H. A. Hancock, the hairs being laid between two glass plates and touched with a flame which burns them away to the edge of the plates.

Harmonic Analyses.—See Periodicity Measurements.

Harp.—See Periodicity Measurements.

[1] W. L. B., *Raw Cotton*, p. 187.

Impact Tester.—Many existing impact testers fail to apply their blow at the centre of percussion of the pendulum. My simple design avoided the loss of energy thus involved, and if any accidental circumstance led to a false blow being delivered, the fact was obvious from a distance by a chattering noise.

The pendulum was a circle of one-sixteenth flat brass sheet, of 25 cm. radius. It was suspended from the ceiling at two points on the upper margin of the circle by four thin steel wires about eight feet long, arranged in the usual way so that the disc could swing in its own plane without twisting. There was no constraint against bodily side-swing of the disc about its two points of support, so that a false blow would start such swinging.

The disc swung between a pair of vertical triangulated brackets firmly anchored in a wall, and could be held back between them or released, by an electro-magnet. A calibrated extension-arm on one bracket stretched forward to indicate the length of the outward swing, by a pointer attached to the disc.

A hole in the centre of the disc received a pin which secured the front clamp of the tested yarn, while the back clamp was in the form of a cross-piece. This back clamp was arrested in the apices of the two triangle brackets when the neutral point of the swing was reached. To make room for these clamps, and for the yarn, a narrow slot was cut radially and horizontally in the disc, nearly reaching the central hole; the piece of metal removed was replaced after being turned through a right angle so that it could be fastened to the lower edge of the slot, and serve as a platform on which the yarn and back clamp rested. A small fraction of the original weight of the pendulum was lost when the back clamp was left behind in the brackets, on breakage of the yarn; this was allowed for in the calibration.

The accuracy and sensitivity of this inexpensive arrangement were remarkably good.

Lea Test.—See p. 233.

Length, of Hair.—See Sorter.

Magazine Hair-break Tester.—Attention has been specially directed to the deficiencies of this apparatus in Chap. XV, but these deficiencies are inherent in dead-load testing of single hairs, and apart from the time required in making such tests, this apparatus is quite serviceable. It is designed to economise as

much time as possible, the loading of the magazine and the computation of the previous test being carried on while the following magazine is being automatically tested.

An appliance for semi-automatic hair-break tests was described in *Raw Cotton.*[1] It was subsequently much modified into a magazine type of instrument, and in another modification was simplified to a purely mechanical design which has been in routine use for seven years. It has been exhibited,[2] and copies supplied to other laboratories.

The length tested is standardised at 1 cm., and the rate at which the load is applied is also standardised roughly, to be comparable with the rates of loading used in single-thread testing with the Yarn Dynamometer (*q.v.*) and in the Lea test.

The tension is applied by a pendulum with a quadrant behind it, so that the radius on which the pull is applied to the pendulum, by means of a silk thread lying along the curve of this quadrant, always remains constant whatever the deflection of the pendulum may be. The pendulum swings in frictionless cup-and-vee bearings on steel points, and its movement is recorded by a high-tension spark which burns a trace in the paper on the recording disc; for knowledge of both these devices I am indebted to the directors of the Cambridge Instrument Company. These precautions against friction are very necessary when the average breaking-load to be measured is only a few grams, and the variability from hair to hair is very great.

The pendulum is mounted on a carriage, which is operated along a geometric slide by a screw and nut, the rotation of the screw being controlled by an automatic reversing gear. The nut on the carriage, whereby it is moved, has a floating mounting, so that defects of the screw cannot cause the carriage to bind on its slides. The reversing gear which drives the screw has no neutral point; it must either drive forward or backward as long as the driving motor is running.

Thus, when the carriage is drawn away from the point where the silk thread of the pendulum is attached to a hair, the pendulum rises until the hair breaks. After the maximum likely tension has been exceeded, reversing gear brings the carriage back in order that the pendulum may be attached to the next hair on the magazine. This attachment is made by a suspended hook

[1] p. 190. [2] Roy Soc. Conversazione, 1924.

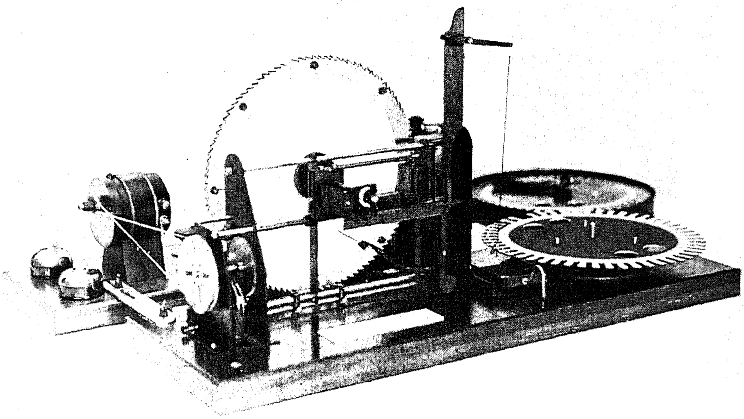

FIG. 126.—The Magazine Hair-break Tester. The carriage, with its pendulum and the recording disc, is in the forward position, and the hook is down. The sliding "control bar" is the lower one beneath the recording saw-toothed disc. The miniature power-plant is at the back on the left. The magazine mounting-frame stands behind the magazine itself on the right. Only a dozen brass eyelets are left on the magazine.

[*To face p.* 356.

of sufficient length to remain effectively vertical while the hair extends under load; this hook is fastened to the silk thread on the quadrant of the pendulum. The hook is lowered automatically at the end of the return journey of the carriage, being thereby placed in a vee-guide so that its lower end enters a hole in a light piece of brass foil, to which the front end of the hair is fastened by a suitable wax (see "*Hair Break*"). The rear end of the hair is similarly waxed to the magazine.

This magazine is a disc which carries 58 hairs in slots round its edge, the open end of each slot being bridged by the loose pieces of perforated brass foil. The end of each slot is shouldered, and the edge of each piece of foil is turned over, so that the two fit together, registering the piece of foil in correct position. The magazine rotates in a horizontal plane, presenting a fresh slot on each return of the carriage.

This movement of the magazine is intermittent, as also is the rise and fall of the hook, and these two, as well as the reversal of the gear, are all secured from a single "control bar" which is moved endways through a short distance by a fork which projects below the carriage and engages with one or the other of two adjustable stops on this bar.

The movement of the carriage takes the recording disc past a fixed pawl, which turns the record on one step further for each hair tested. The separate records for each hair thus obtained can be compared with the broken stump of each hair left on the magazine.

Mounting of the hairs on the duplicate interchangeable magazines is facilitated by a special stand and clamp, so that the loading can be completed by expert operators in half an hour. A quarter of an hour suffices to read and plot the previous record, by which time the previous magazine has been finished.

Thus the actual testing is free from operative errors, and can go on continuously if desired. The use of a pendulum involves the inertia error which Dr. Shorter has identified, and a spring would be preferable in a re-design.

A miniature "power station" is made for the appliance, of components used on autocars and in wireless, so that the spark and the motor drive can be obtained and regulated off the six-volt cells of a Ford car.

Periodicity Measurements.—An account of the appliance

called the " Harp " has been published elsewhere.[1] It served as a Periodoscope and a simple Harmonic Analyser. The curve to be studied is cut out of a strip of cardboard, and this templet is pushed under a number of equidistant stretched strings, the average deflection of all the springs at any moment being shown by a spot of light reflected from a mirror on the yoke to which all the strings are fastened. As the strings radiate from a common centre on the yoke, the templet can be examined with various string-to-string intervals, by choosing various distances from the yoke at which to traverse the templet.

In using this apparatus for the study of diameter variations in yarn, I discarded photographic methods for drawing the templet in favour of direct observation. A microscope was arranged with a peg in its field, over which peg the yarn was drawn; one side of the yarn was thus always at the same point, which was made the zero of a micrometer scale in the eye-piece; only one reading on this scale was needed in order to record the diameter. The observer at the microscope could move the yarn through the field in short steps by turning a handle, which was connected through Meccano gearing of variable ratios to the winding drum of the yarn on the one hand, and to rollers which fed the strip of templet-cardboard on the other. A given length of templet could thus be made to correspond to any required length of yarn, the required expansion or contraction being provided by the gearing.

A second observer sat by a guide through which the templet-cardboard passed. On this guide was a graduated scale, and this observer dotted points on the moving cardboard, up and down this scale, corresponding to the measurements in the microscope announced by the first observer. These points were afterwards joined into a smooth curve, which was then cut out to serve as a templet.

Several yards of templet could thus be prepared in an hour or two. Photographic methods had previously been employed with similar apparatus, but the bromide paper film strip gave too small a templet for convenient use, and the method was slower, as well as being costly and troublesome.

Prior to the development of this technique I had discarded a

[1] W. L. B., *Proc. Roy. Soc.*, A, 1921.

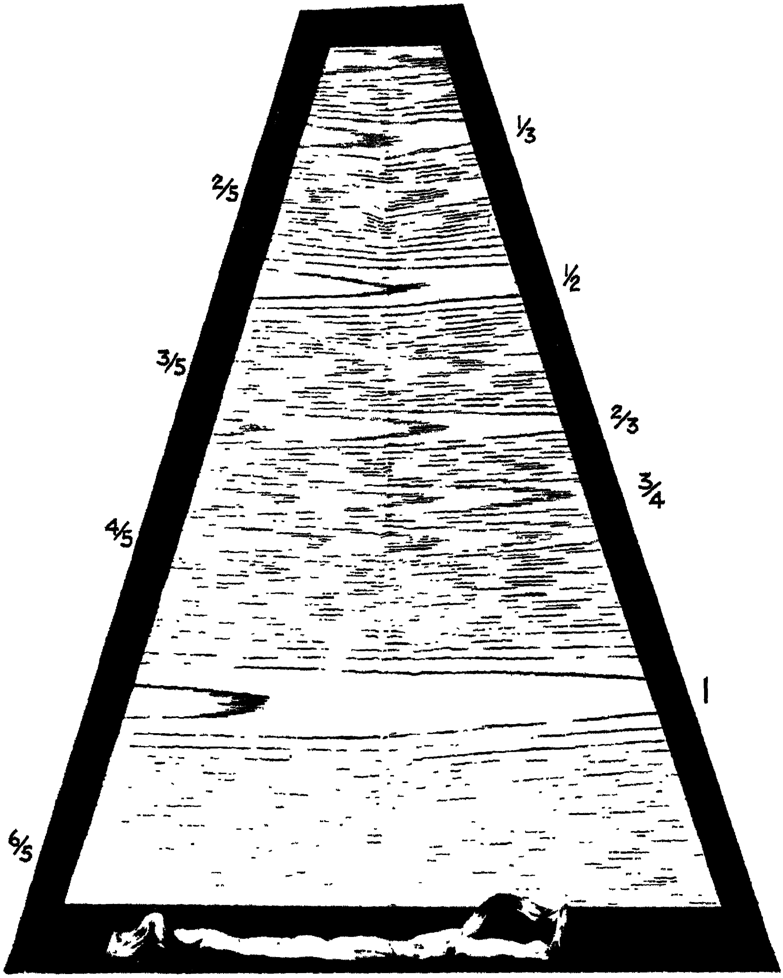

FIG. 127.—Demonstration of the use of Cone-winding for the detection of periodicities.

A skein of yarn, like that shown at the foot of the frame, had black marks made on each end of it. The yarn was then wound round a tapering board in a lathe, and subsequently cemented into a sheet by spraying with collodion. This sheet was then cut, removed, and unfolded as the flat sheet here photographed.

The bold single mark of the unit period stands out clearly. Above and below it can be seen many other patterns at fractional periods.

[*To face p.* 359.

method which has some interest, though it was unsatisfactory for yarn studies. This was termed Cone-winding, combined with colour-marking of the yarn.

The yarn was dragged through a V-shaped guide of glass, which was set slightly out of line with the yarn, so that the latter pressed lightly into the V. Thin places dropped in further than thick ones, and the momentary changes in position this effected as the yarn ran through the guide were recorded on the yarn itself by two syphon pens of differently coloured inks. The pens were so placed that when the yarn dropped to the bottom of the guide it touched one colour; when it rose towards the top it touched the other colour; in a middle position it remained white, equidistant from both pens. Thus thick places were automatically marked red, thin ones green.

This marked yarn was then submitted to Cone-winding, which consisted in winding it as a single layer upon a conical mandrel. If there is a periodic succession of marks among those made on the yarn, these will coincide at the appropriate circumference of the cone, leading up to this point by overlapping, and away from it by underlapping, so that an arrow-head mark is seen on the cone. Similar and related patterns are formed less boldly at various fractions and multiples of the period, at other parts of the cone. Marks which are not periodic produce no regular pattern, but only a general blur. For such reasonably simple cases as often arise in the examination of defective textile goods this method is quite useful.

Porometer, for Hair Cross-section Changes.—This remarkably simple and accurate method was suggested by a casual reference to porosity testing of fabrics, which recalled me to previous experience in the use of Sir Francis Darwin's Porometer for measuring the aperture of stomata in leaves,[1] and my automatic modification thereof, called the Stomatograph. It was found practicable thus to measure the variations in the air-ways left between parallel hairs, by plugging a suitably sized hole with a suitable hank of sliver; hence the changes in hair cross-section were inferred by difference. Exhaustive studies by Mr. Goshawk have shown that the measurements thus inferred are genuine.

Cyclic data can be obtained by the original simple porometer of Darwin, provided that temperature and humidity are carefully

[1] *Loc. cit.*, p. 51*a*.

controlled. Time curves, showing the reaction to change of conditions, can be taken with the Stomatograph pump and recorder.

Rigidity of the Hair.—This was measured by Searle's Method, suitably modified to small dimensions by Mr. Slater for dealing with hairs. The hair tested is placed so as to cross the centres of two duralumin bars, which are slung on single-filament silk suspensions, the plan view being that of a letter H, with the hair as the cross member. The bars have spots of electrolytically deposited iron on their tips, so that they can be set into oscillation by alternating current magnets. The period of their free oscillation measures the rigidity of the hair in respect to bending.

Torsional rigidity can be obtained from the same set-up after removing the bars from their silk suspensions.

Single-thread Yarn Tests.—See Yarn Dynamometer and Automatic Single-thread Tester.

Sledge Sorter.—See Sorter.

Spinning Tests.—See *Handbook of Spinning Tests for Cotton Growers*, London (Macmillan), 1920.

Spinning Yarn, Vibrations of, q.v.

Slave-string Measurement of Yarn Tension.—The purpose of this method was to obtain measurements of the tension upon yarn while it was actually being produced in the spinning machines under mill conditions. In developing the method a large share was taken by Mr. Goshawk.

As applied to the mule it was more elaborate than in the ring-frame. One end of a silk thread, the "slave-string," was fixed to a bracket so that it coincided with the tip of an idle spindle. The other end followed the normal path of this spindle's yarn between the fallers, up to the roller nip; at the roller nip the top roller was removed, and the string just grazed the bottom roller to pass under a pulley, whence it was led to the back of the creel and another pulley, from which its end hung down with a known weight on the end.

The spinning yarn on the next spindle thus had this slave-string running exactly parallel to it, under known tension, at all stages of the mule's cycle of operations, and the inertia of the moving weight was only involved in the tension during accelerations. This strand of yarn was then linked to the slave-string by a very light double hook, the hook being slightly shorter than

the distance between the two, so that it would move sideways towards the one in which the tension was greater. A suitable guide placed some three inches in front of the roller prevented this hook from moving forwards with the yarn, or backwards when the slave-string ran back during winding.

The lateral movements of the hook were observed in a micro-telescope, thus indicating the tension on the yarn directly, when the slave-string tension was known. A more convenient null method could be used by varying the slave tension, and noting with a stop-watch the times during the mule-cycle at which the tensions were in equilibrium, as shown by the index-mark on the hook passing through the central position.

The application of the method to the ring-frame was comparatively simple, and the actual tension above the thread-guide was measured with errors of only about 1% under all sorts of conditions. Very fair tests of the efficiency of traveller lubricants could be made in this way.

Sorter.—The " Sledge " pattern of this appliance has been very fully described elsewhere.[1]

Essentially the sorter mechanism consists of a pair of delivery rollers which deliver cotton through their nip, while at the same time they are being translated bodily in relation to a collecting device, such as the strip of plush used in the sledge pattern. The rollers are fed with hairs which have previously been laid parallel in a sliver or lap, and the feed is intermittent in a series of cycles. For any one cycle the sliver is fed forward about a millimetre by the feed rollers which support it; it is then brought up to the nip of the delivery rollers and held there while they are turning through the feed-distance; thus the delivery rollers take hold of the front ends of a number of hairs. The sliver is then withdrawn quickly to its former position about an eighth of an inch away from the delivery roller nip, so that the particular hairs which had been gripped are partly dragged out from the body of the sliver. The delivery rollers continue to turn uninterruptedly, so that these partially freed hairs continue to be dragged out and are passed forward to the other side of the delivery roller nip, still with their front ends level, until all of them have been set free from the rollers and gathered up by the collector.

[1] W. L. B., " A Method for Measuring the Length of Cotton Hairs " (*Handbook of the Sledge Sorter*, London, Macmillan), 1921.

Since the front ends are in alignment, it follows that the first hair to be released on the other side of the delivery rollers will be that hair which was shortest. The longest hair will be the last set free. Since the relative movement of the collector is synchronised with this cycle, any one part of the collector will always receive hairs of the same length. Thus the mixture of hair lengths in the original sliver or lap is "sorted" into a regular sequence of lengths, which may either be continuous or be subdivided into arbitrary groups, as convenience may dictate.

The accuracy depends principally on three factors: the straightness and parallelism of the hairs, the instantaneous collection of each hair when released, and the absence of any hair movement except on the part of those hairs which have actually been gripped by the delivery rollers. The first is easily controlled, and the second has been effected at a speed of 300 cycles per minute, but the third is a source of inaccuracy, known as the "hair-displacement error," and discussed as such in the main text (pp. 64, 143).

Various forms have been assumed by the appliance, since its first primitive model, made from half a dozen Meccano parts. The automatic forms began with a reciprocating movement along the line of the roller-axes, and then a continuous circular movement which was more sensibly rearranged in the fourth model to take place in a plane at right angles to the rollers. In all the feed-box and delivery rollers moved, while the collector was fixed. A re-design was then carried out, with an important contribution from Mr. Hutchins, to make a portable form which could be used in camp if necessary; this was entitled the "Sledge Sorter," and is now manufactured by the Cambridge Instrument Co.

From the earliest stages another re-design had been contemplated, as a machine for producing adequate quantities of sorted cotton. The whole device was too far off the lines of ordinary textile practice to be built by the usual machinists; in fact both the British and German patent offices had at first refused the original Sorters a patent on the grounds that the method of operation was not disclosed! The workshops of the Experimental Department were therefore developed (p. 344), and Mr. Hutchins proceeded to construct a machine which would sort a ten-inch lap. We had satisfied ourselves that speeds of

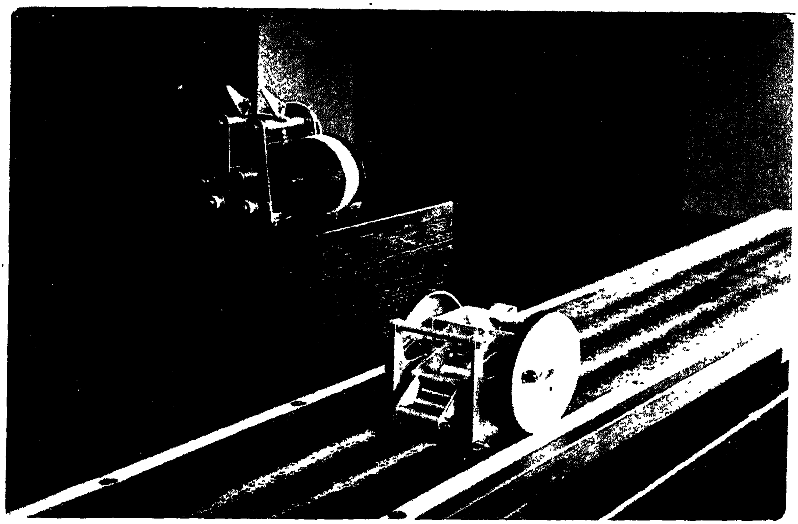

FIG. 128.—The Sledge Sorter, showing the sledge standing midway along the plush-covered board of the collector, on which are two deposits of sorted hairs between the two calibrated tapes. In the background is the set of easily adjustable drafting rollers used for preparing the sliver, mounted on the case of the instruments.

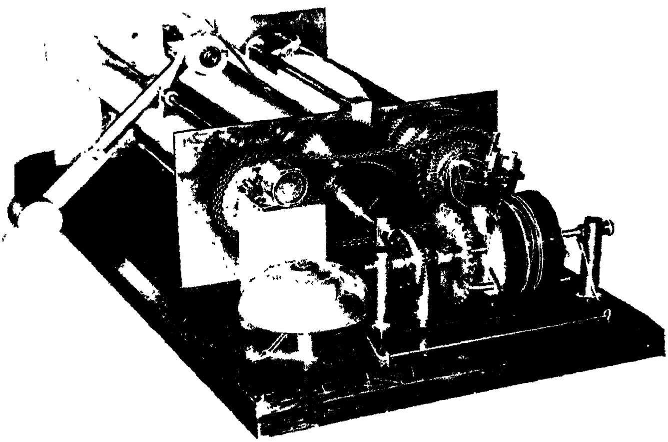

FIG. 129.—The Unspinner, for turning yarn back into sliver. The cop to be unspun is in the revolvable cradle on the right at the far side. The delivery rollers at the near end of this cradle deliver sliver on to the plush-covered roller at the right-hand front corner. The amount of untwisting is regulated by moving the belt and jockey pulley seen on the left.

[*To face p.* 362

production comparable with those of the comber were possible, so that cans of sliver could be collected at reasonable rates, each can containing one group of lengths of staple.

It took a large part of three years before the Machine Sorter was sufficiently satisfactory. The details are of no general interest, but a few citations will show the variety of problems which had to be faced: the construction and covering of delivery rollers which would grip firmly on single hairs at any point in all positions along a ten-inch nip; the removal of each free hair and its deposition on the appropriate section of the collector, when the time during which that section was operating might be only a fiftieth of a second; movement of the feed rollers for one millimetre forward, followed by complete arrest, again with only about a fiftieth of a second in which to do it; synchronisation of the movements of the plate, which opened and closed so as to permit the feed to come through and yet controlled the hair-displacement error. Many minor difficulties were overcome.

There is little doubt that sorting for length is practicable. The existing machine is not the only workable arrangement, and now that most of the difficulties have been evaluated, another re-design might be undertaken in order to avoid them.

Torsionmeter, for Yarn.—This appliance dealt with a suspended length of one metre of yarn, the upper end of which was fixed to a revolving hook on a toy motor, as in the Twist Yield Tester (*q.v.*). The lower end was attached to the centre of a light aluminium bar, hanging horizontally, which served as the torsion index.

This was effected by suspending below this bar a second bar placed parallel to it at a distance of 50 mm., the suspension being by two parallel silk filaments set 10 mm. apart. With a lower bar weighing 1 gram this construction was suitable for 100s yarn. While the upper bar was thus free to rotate as an index through a limited arc, the lower was prevented from doing so by contact with two vertical parallel stretched wires.

Both bars were freely suspended from the yarn, and could therefore follow the expansion and contraction of the yarn with changes of the twist, and a parallax mirror thus enabled measurements of length changes to be made concurrently.

The load on the yarn could be adjusted to any value above

the minimum of 1 gram by adding weights at the centre of the upper bar, without affecting its function as a torsion index.

The addition of a revolution counter and a reversing switch to the toy motor enabled any number of turns to be added or subtracted at will.

Twist, in Yarn.—See Clinometer.

Twist-yield Tester.—This was developed from the simple and effective device of suspending a weight from a length of single yarn, and counting the revolutions of the weight until breakage.

The length of yarn, one metre or less, is suspended from a hook which can be rotated in either direction by a toy motor. On its lower end is fastened any known weight, in the form of a bar which hangs horizontally, and is prevented from rotating with the yarn by contact with two adjacent vertical guide wires, as in the Torsionmeter (*q.v.*). When the yarn has been sufficiently untwisted or over-twisted by the motor, it breaks and the weight drops.

The dropping weight falls upon a metal plate which is lightly wedged between the two guide wires, and knocks it away. As the current is taken to the low-voltage motor through these wires, using the plate as a plug-switch, the fall of the weight stops the motor. A revolution counter shows the number of turns added or subtracted.

By making use of a progressive series of weights, a curve can be plotted to show a twist-strength relation under the conditions of the test.

Un-spinning, of Yarn.—When large numbers of hairs are required for the examination of the constituents of yarn, as in sorting, the yarn can be untwisted to make a sliver in an automatic appliance called the " Un-spinner."

The cop or bobbin is placed in a cradle which is provided with a pair of delivery rollers at one end. This cradle is then rotated to take out the twist from the yarn as it is fed out of the cradle through the delivery rollers. The untwisted yarn is wound up on a plush-covered drum immediately, the surface speed of this drum being so geared as to be slightly greater than the surface speed of the delivery rollers. By the use of differential gears these speeds are permanently coupled together, while the cradle revolutions can be changed without affecting them; the number of turns removed for various yarns can be varied by the

single operation of shifting a belt along a pair of cone pulleys. This belt control is a defect, on account of variable belt-slip, even under the constant tension provided by a jockey pulley.

The irregular distribution of twist in the yarn necessarily leaves an irregular untwisting in the sliver produced, but not enough to prevent it from being worked up eventually into a good sliver by the use of the draw-box provided with the Sledge Sorter.

Vibrations of Spinning Yarn.—The study of the behaviour of yarn when spinning is hampered by the accumulation of twist therein. The simple device of fastening a weight to the end of the yarn, and allowing it to spin also, enables continuous observations to be made upon yarn which has been brought into a variety of twist conditions. The twist accumulated in the length of yarn depends on the air-resistance of the weight, and the weight itself may be varied independently of the air-resistance. The arrangement is most convenient when the yarn is horizontal, and the weight hangs down over a smooth hook of duralumin, which has a low coefficient of friction.

Wall Thickness, of Hair.—See Cross-section.

Weight of Hair.—See Hair Weight.

Workshop Equipment.—Mention having been made (p. 332) of the value attaching to a laboratory workshop, and the cost of such a workshop being quite elastic, it may be useful to set out a list of equipment which can undertake a very wide range of experimental construction and repair, at a capital cost lying between £50 and £100 according to the type of lathe selected, and the quality of some of the other tools. Sizes are indicated where they are relevant.

Lathe.—Wade; Drummond model-makers; Drummond $3\frac{1}{2}''$; or Star $4''$.

Slide-rest tools, boring bars, knurls, small grinding wheel.

Three-jaw self-centring chuck, tail-stock drill chuck.

$\frac{5}{8}''$ and $\frac{1}{8}''$ end mills, slitting saw, mandrels.

Small table vice, lathe dog, holding-down bolts.

Drilling tools.—0–70 wire gauge twist drills.

$\frac{1}{16}''$ to $\frac{1}{2}''$ by sixteenths twist drills.

$\frac{15}{64}''$ twist drill for O.B.A. clearance.

Yankee hand drill with R and L ratchet, for $\frac{3}{8}''$.

Striking tools.—3-lb. ball-pane and ¼-lb. hammers.
Centre punches and nail punches.
Bench anvil.

Holding tools.—20-lb. bench vice.
Spanners; B.A., Whitworth, and adjustable.
Pliers; Bernard vice-pliers, wire-cutting, and round-nose.
Screwdrivers; watchmaker's, ⅛″, and ¼″.
Forceps.

Screwing tools.—0, 2, 4, 6, 8, and 10 B.A. taps and dies.
Stocks of bolts, nuts and washers in B.A. sizes.

Cutting tools.—Wiss shears; curved-blade small snips.
Hack-saw, 10″, 18 and 10 tooth blades.
Scissors, cold chisels, scrapers.
Files; 4″, 6″ and 10″ files in flat, round, and half-round, bastard and fine cuts. Watchmaker's files.

Measuring tools.—Steel tape, 12″ and 3″ steel rules (metric and inch).
Feeler gauge. Drill gauge plate.
Internal and external callipers. Screw compasses.
3″ square. Scriber.
Micrometer opening to 1″, metric or inch.

Gear-cutting tools.—None, use being made of Meccano and clock gears.

Grinding tools.—Bench grinder (and wheel on lathe).

Soldering.—2-lb. and ¼-lb. bits.
Gas or Primus lamp and blowpipe.
Flux and solder.

Woodworking tools.—Bench with vice and stops. Cramps.
Brace and bits, various. Expanding centre-bits.
Square and gauge. Lino knife.
Screwdrivers with long shank, ¼″ and ½″.
Hammer, claw-head, 1 lb.
Chisels; 1″, ½″, ¼″ and ⅛″ mortising.
Gouges; 1″ and ¼″.
Saws; 3″ tenon, large and small rip-saws.
Planes; jack, smoothing, and small iron.
Scrapers. Leather mallet.

Yarn Clinometer, q.v.

Yarn Diameter.—See Periodicity Measurements and Clinometer.

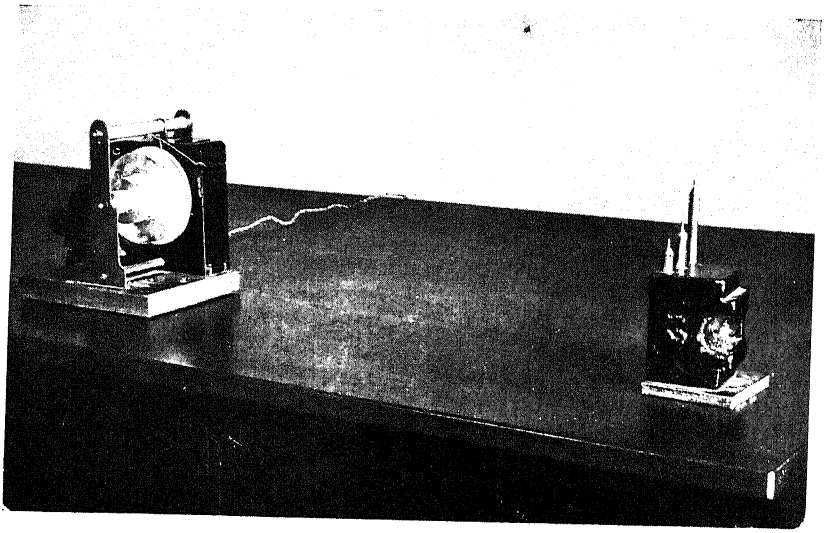

FIG. 130.—The Yarn Dynamometer. Showing the drum with its three steps, and the extension disc on its far side, with pawl and faller wire above; the latter has not yet been raised clear by the yarn on which it rests. The dynamometer gear is in the casing at the back. The triple-calibrated scale is concealed behind the drum itself. The heavy weight with three pegs, whose heights correspond to those of the drum-steps, serves to anchor the outer end of the yarn.

[*To face p.* 367.

Yarn Dynamometer.—The need for an easy means of varying the length tested was the dominating consideration in the design of this appliance. All the operations were performed at one end of the yarn, the other end being fixed. A revolving drum wound up the yarn, and at the same time indicated the load thereon by means of a dynamometer attachment through which the drum was driven. Any length whatever could thus be tested, if the rate of revolution of the drum was regulated to give a constant percentage rate of strain at every length. It should be noted that the length of yarn actually tested remained constant, because any extension was wound up on the drum.

The cast aluminium drum had three steps, in order to widen the range of loadings through which the instrument could be used; a corresponding triple calibration was provided on the dynamometer scale. For routine use on lengths of 8, 4, 2, 1 and ½ metres a pair of five-step pulleys were provided on the initial drive from the motor, so that the rate of revolution of the drum could be halved or doubled after each test by simply shifting a rubber driving band from step to step.

The pendulum of the dynamometer proper, containing a train of clock-gears, was slung on the same axle as the drum. A large gear-wheel on the drum was driven by a pinion on the pendulum, at the end of its train of gears; the beginning of the same gear-train was a pulley concentric with the common axle of drum and pendulum, and rotated by a belt at high speed from the shaft of the driven step-pulley. The reduction was enormous, so that no appreciable turning moment was applied by this pulley to the drum, even at maximum load. As the drum rotated it wound up the yarn, and the pendulum rose to show the tension. A fixed pawl engaged with a moving rack attached to the pendulum, so as to prevent the index of the latter from falling back when the yarn broke.

It may be noted that the usual arrangement of a fixed rack and moving pawl is wrong, since it introduces a variable error.

Total extension at break was recorded by a disc beside the drum, and concentric with it. This disc was held to the drum by friction on a flannel pad by a light spring, and turned with it, unless the milled edge of the disc was locked by the weight of a pawl; this " extension pawl " was attached to a wire and nearly counterpoised, so that the wire and pawl were lifted by the yarn as the catenary loop of the latter straightened out

in taking up the load. This released the extension disc, so that it could turn with the drum until the breakage of the yarn allowed the pawl to drop again and lock it. The disc was also triply calibrated.

Length-marking, for subsequent determination of count by weighing, was effected automatically when required by simply setting a fine brush charged with coloured ink slightly below the line which the yarn took when in tension. As the catenary loop flattened out it dragged the yarn past the brush and marked off a length under constant tension.

Clamping of the yarn to the fixed support, and to pegs pushed into holes in the periphery of the drum, was done by knotting; this method was eventually proven to be less uncertain than any form of clamp. The knots were made with a crochet hook.

The instrument has defects, although it has been invaluable. It is not automatic, and since the drum has necessarily to perform complete revolutions there is too much friction involved in its circular pivots. The small ball-bearings of a magneto were used, but these were still too sticky to respond with invariable freedom to minute changes of tension.

The importance of eliminating sampling errors, by working through all the lengths in succession, has been emphasised in the main text. Very short lengths should be tested in suspended clamps, attached to the instrument by stronger cords, so as to avoid the possibility of breaks taking place on the curve of the drum.

Yarn Elastometer, q.v.

Yarn, Spinning Vibrations of.—See Vibrations.

Yarn Torsionmeter, q.v.

Yarn Twist.—See Clinometer.

Yarn Twist-yield Tester, q.v.

Yarn, Un-spinning of, q.v.

Yarn, Vibrations in Spinning of, q.v.

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